MSS. and other Communications for the Editor should be addressed to
Prof. G. RYLE, Magdalen College, Oxford.

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MIND

A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY

EDITED BY
PROF. GILBERT RYLE
WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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I.—CAUSES OF BEHAVIOUR AND EXPLANATION IN PSYCHOLOGY

BY P. C. DODWELL

THE fact that we can successfully predict how people will behave in certain situations does not imply, much less entail, that we can explain their behaviour; and the fact that we may have an explanation for some piece of behaviour does not necessarily imply that we can predict how a person will behave in a given situation with any degree of assurance. Being able to predict a person's conduct in a situation successfully may be connected with having an explanation of why the person behaves in the way he does, but the connection is not a necessary one. This is so not only in the trivial case where we might say "Jones always stoops to tie his shoelace before crossing the road, but I can't think why"; Jones does something which seems irrelevant so far as any of the normal means of explaining the way people behave is concerned, yet we may in fact be able to predict quite successfully what he will do whenever he finds himself in the position of having to cross a road. We should be liable to fall back in such a case on what a Freudian might consider a convenient cloak for ignorance—we might explain that it is simply a habit of Jones's. A Freudian zealot might search for an explanation in terms of repressed anxiety, traumatic childhood experiences of road accidents or whatnot; in many cases he may be quite successful in making out a case for such an explanation, but this does not affect the logical point that being able to predict behaviour does not have any necessary connection with being able to explain it. The acceptance of Freudian theories merely disposes people to widen the class of activities for which it is thought that explanations in terms of purposes and intentions—albeit unconscious

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ones—are appropriate. (See MacIntyre, *The Unconscious*). The range of phenomena which has to be taken into consideration in order to make a prediction is not necessarily coextensive with the range which has to be taken into account in order to give an explanation.[1]

Peters has recently argued, in *The Concept of Motivation*, that explanations of human conduct in terms of a "purposive, rule-following model" are the only appropriate sort of explanation to give except in cases where there is obvious reason to think that a person is deviating from an accepted set of standards for behaving in some situation. Only when this happens, Peters maintains, does it make sense to ask what *caused* a person to behave in the way he did; only in situations where, through physical or some other sort of constraint, a person behaves out of character—as it were in spite of himself—is it appropriate to look beyond the purposive model to predisposing factors which make, force, or "cause" the person to act in the way he does. If we are successful in identifying such constraints, we can still only "explain" anomalous behaviour with their aid if we have a theory about how those constraints affect behaviour—a theory like Freud's. It should be emphasised that in such "explanations" we still use the language of the purposive rule-following model, whether implicitly or explicitly. Peters makes the objection to many so-called theories in psychology that they attempt to divorce the contraints, predisposing factors or antecedent conditions from the conceptual requirements of the model and talk simply in terms of mechanical or quasi-mechanical forces as the sole determinants of behaviour. Neglecting the logical requirements for an explanation of behaviour, he holds, leads to a logically inappropriate method of attempting to explain behaviour. Peters is not denying that physical processes are necessary conditions of an organism's behaving in one way or another, only that such conditions could never be sufficient, taken on their own.

This attack is directed against all psychological theories which attempt to explain behaviour in terms of physiological states, organic processes, neural circuits and the like. It is an attack which irritates psychologists a great deal. The reason for the feeling of exasperation is, I think, this: It is manifestly the case

[1] I think the sense in which I am using the word 'predict' is clear enough; predicting from a theory is quite a different matter and is obviously not independent of theoretical (and hence explanatory) concepts. *Cf.* Peter Alexander, "Theory-Construction and Theory-testing", *British Journal for the Philosophy of Science*, vol. ix, No. 33, 1958.

that changes in the organic state of the body, *e.g.* through the administration of drugs, affect behaviour. A great deal of psychological research in recent years has been directed just to this end, of investigating the effects of organic changes on behaviour. Most psychologists regard the results of such experiments as interesting and important. Behaviour changes *predictably* under known changes in organic conditions; from this they tend to make the assumption that, given enough information about organic states, a large number of predictions could be made about consequent behaviour, and that it is merely our ignorance of the organic states which prevents us from being able more fully to predict behaviour. If identifying a certain organic state is sufficient to predict a particular piece of behaviour, what more, the psychologist might argue, do we need in order to be able to explain it? In principle, he feels, if we knew enough about the physiology of the body we should always be able to predict with confidence how people would behave: this being so, what other phenomena could be relevant to an explanation of behaviour other than those which are sufficient to predict it? Why bother with the logical hair-splitting of philosophers? To put the matter another way: if we knew enough about the processes going on in the body—particularly in the brain—then we should be able to *specify* sufficient conditions for the occurrence of a particular piece of behaviour. As a matter of fact it might indeed one day happen that neurophysiology will be so far advanced that this will be the case. This seems to dispose of an argument used by Hamlyn (*The Psychology of Perception*) that, because of the logical differences between the normative language we use to talk about perception and the language in which we describe physical processes in the body, it would never be possible to specify sufficient conditions, in terms of physical processes, for *perception* to occur; one could never deduce a statement about perception from a set of statements about brain processes and the like. (Hamlyn indeed establishes more than this, and the argument applies not just to perception, but to behaviour in general; for the points I wish to establish it is sufficient to note that there can be no deductive relationship between statements about physiological processes and statements about ensuing behaviour, which is but a variant on the well established logical theme that one cannot deduce a statement about what *ought* to be the case from statements about what *is* the case.) What puzzles psychologists about this argument is the brute fact that it is quite conceivable that one day a psychologist or physiologist will be able to say "As a matter of fact, whenever

I can identify such-and-such a set of processes in the intact organism, it always follows that it behaves in such-and-such a way. I have identified a set of processes which are demonstrably sufficient conditions for the organism to behave like this." This is at least one of the things which make it so hard for psychologists to accept the philosophers' thesis. There is obviously a difficulty here, and it seems to be rooted in a confusion about the uses of the phrase "sufficient conditions", a confusion which can be resolved by pointing to two quite distinct uses, one in the context of predicting, the other in the context of explaining. As I pointed out earlier, being able to predict behaviour does not imply being able to explain it.

In saying that the effect e is caused by the conjunction abc . . . we do not necessarily assert anything about the nature of e, although we imply that it is an observable, unambiguously identifiable event or occurrence. In particular the statement that whenever abc . . . occur conjointly e also occurs does not entail that abc . . . is connected deductively with e; it need say nothing about the logical relationships of abc . . . to e. It could be simply a statement of an empirically observed dependence; in one accepted sense of the words "sufficient conditions" we can assert that a b c . . . are sufficient conditions for the occurrence of e; it would also be an accepted usage of the word "cause" in this case to say that abc . . . cause e. We have correctly predicted e, and stated the grounds we have for predicting it in terms of a general law.

A recent finding in physics provides an example of this use of the words "sufficient conditions" in a context where an effect is predictable from a well-defined set of antecedent conditions. It has been found that in a vessel containing water vapour at a certain pressure and within a narrow range of temperatures "plate-like" snowflake crystals are formed on a nylon thread suspended in the vessel; if the temperature is changed to within another narrow and well-defined range "needle-like" crystals are formed. The physical state of the vessel constitutes a set of sufficient conditions for the formation of a particular type of crystal, but those conditions state nothing about the nature of the crystals, nor can the difference between a "plate" and "needle" crystal be *explained* in terms of those conditions; the concept of crystal form has no logical connection with the concepts of temperature and humidity. Yet, in a widely accepted use of the word, we can say that the given situation in the vessel causes the formation of one type of crystal or the other. There is as yet no theory to explain adequately why one type of crystal is

formed under one set of conditions, a different type of crystal under another.

So we can accept it that a given set of conditions may be sufficient for the prediction of an effect although there is no logical connection between the initial conditions and the outcome. "abc . . . cause e" asserts a causal relationship; does it constitute a causal explanation? In the case just considered it clearly does not, but in other contexts it may do so. Before pursuing this point further let us consider a different sense in which we can talk of sufficient, and in this case also of necessary, conditions for something to be the case.

In mathematics, especially in that branch known as analysis, the subject is developed in terms of necessary and sufficient conditions for a function, or a whole range of functions, to have a certain property. This is done by deducing that if a function has a certain property α this can only be so if it also has a certain other property, or set of properties, $\beta_1 \ldots \beta_n$. Thus $\beta_1 \ldots \beta_n$ are necessary conditions of α. They are shown to be sufficient (perhaps in conjunction with another set of properties γ) if, given $\beta_1 \ldots \beta_n$ with γ, the property α is entailed as a property of the function.

Here we would not talk of the properties $\beta_1 \ldots \beta_n$ and γ *causing* the function to have the property α; but I think it would be a reasonable use of language to say that if a function has the properties $\beta_1 \ldots \beta_n$ and γ, and we show that it follows from this that it must have the property α; and that if it has the property α it can only have it if it has (some selection of) the properties $\beta_1 \ldots \beta_n$, then the relationship of entailment between $\beta_1 \ldots \beta_n$ and γ, and α, explain the property α. In general we should expect $\beta_1 \ldots \beta_n$ and γ to be more primitive or basic properties than α, and showing that $\beta_1 \ldots \beta_n$ and γ are necessary and sufficient conditions of α will constitute an explanation of α. It constitutes an explanation because one has shown the logical connection between the properties, and how one follows from the others. Clearly we are using the words "sufficient conditions" here to do quite a different job from that done by the same words in the physicist's example of snowflake crystal formation.[1]

The two cases outlined seem to be diametrically opposed to each other; they are two extremes of a group of ways in which one can talk of sufficient conditions. On the one hand one

[1] The fact that the mathematician can establish both sufficient *and necessary* conditions is of some importance, but does not seem to invalidate any of the arguments put forward here about sufficient conditions. *Vide infra.*

can give sufficient conditions for an event to occur as an empirical generalisation, and there is no necessary connection between antecedents and consequent; on the other hand one can give (necessary and) sufficient conditions in the mathematical sense, where the consequent is shown to be entailed by the antecedents or certain combinations of them and only by these antecedents. In the first case one would not speak of an explanation, but in an accepted usage of the words one could say that the antecedents are the cause of the consequent event; in the second one can say that one has *explained* the consequent in terms of the antecedent conditions or properties, but it would be an odd use of language to say that the antecedents *cause* the consequent.

There are many cases, particularly in the physical sciences, where either of these situations may hold, or both, and where there is an intimate connection between the two. In these situations the sufficient conditions in the first sense will usually be very closely related to, and may seem to be identical with, the sufficient conditions in the second, and for this reason the two are not always clearly distinguished from each other, even in scientific discourse.

A very simple example from elementary physics illustrates the point:—

It can be established by empirical observation that, all else being equal, the intensity of illumination on a plane surface varies inversely as the square of the distance of the surface from a constant light source. From this empirical generalisation one can predict what will happen if, for instance, the distance from the light source is doubled or trebled; one would say that the change in distance causes a change in illumination, but it would surely be odd to say that the change in distance *explains* the change in illumination except in the rather loose sense where one might answer the question of a naive observer, "Look, the brightness has changed, how do you explain that?" with the rejoinder, "Well, look, I have altered the distance between the light and the screen". A sort of explanation of a phenomenon can be given by subsuming it under an empirical generalisation, but this way of explaining is generally held to be not very satisfactory in a science. The change in illumination is explained by reference to a formal model, the sort of system which Professor Toulmin compares with a map. One considers the model in which rays of light travel in straight lines, the source of light is a point source from which a fixed number of rays emerge, uniformly distributed in all directions. Intensity of illumination is defined as the number of rays impinging on a surface per unit area. By

considering a spherical surface with such a point source at its centre one deduces that the total illumination (luminous flux) over the sphere is invariant for changes in its size, since all the rays impinge on it somewhere. But the intensity of illumination on any segment of the sphere of constant area varies inversely with the surface area of the sphere, and hence inversely with the square of the radius of the sphere, *i.e.* the intensity of the illumination varies as the reciprocal of the square of the distance of the surface from the light source. This I think we would normally call an explanation of the dependence of intensity of illumination on the distance from the light source.

The deductions from the formal model are clearly similar to the deductions of the mathematician, but we cannot talk of both sufficient *and necessary* conditions for deducing statements about the observed phenomena of optics from *a* model, only from the particular model being considered. It may be possible to show that for the model under consideration certain conditions are necessary, but it does not follow that they are necessary for *any* model that might be used in explaining optical phenomena, since one cannot set limits *a priori* on the types of model which might be found acceptable. Since only sufficient conditions can, in general, be established, it would be more correct to talk of *an* explanation rather than *the* explanation, although this is not a distinction which is ordinarily observed very carefully. Be this as it may, it is clear that the sufficient conditions for predicting changes in illumination with distance are different from those from which an explanation of those changes can be derived. Sufficient conditions in the first case can be established simply by observing what happens when the distance between light source and surface is varied, keeping other factors constant, and by having a suitable means of measuring intensity of illumination. Sufficient conditions for being able to explain the inverse square law, however, involve showing that the property to be explained is entailed by the properties of the model in the way previously elucidated; it is also necessary to consider whether the basic phenomena of light propagation are such that the model adumbrated can be considered a reasonable model to use to represent those phenomena. As a first approximation the model certainly does give a "good fit" in this case, and is hence a good model to use for explaining the simpler phenomena of geometrical optics and photometry. It is important to realise that setting up sufficient conditions for the two cases—predicting in accordance with the inverse square law, and explaining by means of its derivation from the model—are different processes, although

they may involve to a large extent consideration of the same phenomena. Many, if not all, the sorts of explanation which we normally tend to call "causal explanations" are, I think, explanations in terms of "well-fitting" models.

The distinctions which I have drawn between the two uses of the phrase "sufficient conditions" are important *vis-à-vis* the attacks on psychological theorising mentioned above and the resentment many psychologists feel about them. The thesis of the philosophers, that one cannot give *sufficient* conditions (and therefore not an explanation) of conduct or behaviour in terms of movements, brain states and the like is incompletely stated; clearly "sufficient conditions for the occurrence of a certain sort of behaviour" is ambiguous unless one specifies the sense in which one is talking about these conditions. Equally clearly, I think, from the sorts of argument used and the examples expounded, Hamlyn and Peters have in mind my second use of the phrase "sufficient conditions" in their attacks on psychological theories. We must concede the point that one cannot *deduce* statements involving normative concepts from statements about the organic state of the body, but it does not follow that one cannot make predictions about behaviour, in language using normative concepts, from a knowledge of what is happening in the brain, for, as we have seen, there need be no logical connection between the statement of sufficient antecedent conditions and the statement of their consequent, if all we assert is that the consequent invariably occurs when the antecedents occur. It is the failure to emphasise this distinction, I think, which has made the philosophers' thesis less clear than it might have been, and which explains the resentment and confusion many psychologists experience when confronted with the philosophers' arguments. Psychologists have been guilty of confusing the two senses in which one can talk of sufficient conditions for an event's occurrence, and have hence tended to think that an adequate theory to account for some bits of behaviour can be based on statements of observable dependence without regard for the important logical distinctions which need to be drawn between the conditions for predicting and the conditions for explaining something.

It is perhaps worth noting that in general psychologists, unless they are diehard Hullians, seldom attempt to elaborate theories about human behaviour *purely* in terms of "colourless movements", physiological processes or the firing of neural circuits. Theories about, for instance, memory and perception may postulate systems of mechanical or quasi-mechanical classification, they may mention the processing of information and the

transmission of impulses in nerves, but usually one finds words like "recognise", "perceive", "infer" and "expect" creeping in sooner or later. This suggests that in attempting to set up a model for some psychological process it is not easy to separate the construction of the model from the conceptual framework within which we normally describe and explain behaviour. Is it in fact necessary that the elements of the model should be strictly related to the concepts applied in the ordinary description and evaluation of behaviour? If it is, then no amount of juggling with the technical jargon of information theory alone will result in a satisfactory theory about behaviour. There are psychologists who would maintain that it is not necessary that a strict relationship must hold between the concepts of ordinary usage and the elements of the model in the sense that an element or process of the model must be identified with each of the essential concepts of ordinary usage, if this is taken to imply that the function of the element or process in the model is identical with the function denoted by its correlative concept. They would maintain that functions in the model may be analogous to the psychological processes of "recognising", "inferring" and so on, that the formal specification of these functions of the model is independent of the "psychological" concepts, and the fact that they are nevertheless analogous to psychological activities like recognising and inferring helps us to understand the psychological activities themselves. This is held to be especially true of machines which "learn", "classify" and "remember". Let us note, however, that when we say a machine "learns" or "remembers" we do so because we already know what it means to learn or remember, and using these words is a convenient way of describing what the machine does. Contemplating such a machine would never teach us what it means to remember something correctly, nor could it help us if we wanted to explain to a Martian the difference between knowing and believing. There may be a sense in which one could talk of a machine's "knowing" some things and "believing" others, but this distinction can only be made after we have understood the meaning of these words, and coming to know their meanings involves familiarity with concepts and ideas which are essentially normative, for instance being aware of the difference between inferring something from available evidence correctly and incorrectly. Of course, a machine can be built which will "infer" "correctly" in some cases and "incorrectly" in others (we can decide for ourselves what the criterion of correctness is to be for the machine) but still to talk in this way about the machine is to prescribe a new use of language. To

think that this new prescription can in any way explain psychological processes is a mistake.

It is just because psychological words can be used—or misused—to draw analogies between the things a machine can do and behaviour that the construction of such machines (or their specifications) has an air of plausibility. Undoubtedly many psychologists find it useful to construct models to help them to clarify their thinking about a psychological process; somehow it is comforting to have a model which in some sense "behaves" in ways analogous to the ways men behave. It is possible to make predictions from the specification of such models, and test those predictions against human behaviour. But if we enquire into the explanatory value of such models the matter takes on a rather different complexion. Recalling my earlier analysis, and the fact that psychological verbs like "perceive" and "infer" can be applied to the models only in a derivative sense, it is clear that a model which is specified purely in terms of physical processes like "fire", "switch on", "switch off", etc., and the topology of its elements can never be sufficient to entail any statement about perceiving, inferring or remembering, as these words are normally used in the description and explanation of human behaviour. This is so because of a purely logical requirement about the premisses and conclusion of a deductive argument.[1]

Why, then, are psychologists so fascinated by model-construction; how is it that this way of doing things has come to be accepted as a most respectable way of going about explaining behaviour? I think the main reason is that the species of model we are considering seems to be one which gives a "good fit"; that is, if we accept the maxim that all behaviour is determined by what goes on in the body—principally in the central nervous system—then a model whose outputs ("behaviour") are determined by physical processes which are rather similar to well-known neurophysiological processes is obviously one which may be a "well-fitting" model, especially if those outputs can conveniently be described by using a "debased" psychological vocabulary. The maxim is accepted, implicitly or explicitly, by most experimental psychologists, hence the popularity of model-building. Clearly, however, in the light of what has been established above, such models suffer fatal limitations as means to any complete explanation of behaviour.

Although many psychologists dispute the conclusiveness of the arguments against the indiscriminate use of models, the fact already noted that psychological verbs tend to appear in their

[1] This point is clearly established by Mr. Hamlyn (unpublished paper).

talk about models seems to indicate that *in use* the inadequacy of the purely mechanical type of model is implicitly admitted. And this seems to be an important point. It is not just that the "debased" vocabulary is applied to a model intentionally, with full regard to the conceptual limitations of that vocabulary, but rather that, whilst denying the conceptual inadequacy of their models which philosophers have been at pains to emphasise, psychologists tend to import through the back door just those conceptual factors which they claim to have dispensed with. Now it is clearly unobjectionable to say that a model can be specified in terms of purely physical processes, and that its outputs can be described by using the debased vocabulary of psychological words; it has been shown, however, that this is not sufficient for the psychologist's purpose, which is, manifestly, to give an adequate explanation of human behaviour. Hence this procedure is bound to be misleading. This is not to assert, of course, that there are not physiological or other physical events which are necessary conditions of behaviour, and it may well be that important things about those conditions may be discovered by the model-building psychologists.

It is appropriate to ask at this point if the system-builder can define the elements of a model not just in terms of physical processes and relations, but in terms of psychological antecedents, *i.e.* can the elements and processes of the model be defined by reference to some one or more of the normative concepts which we employ in the description of behaviour? Not only is this possible, but it seems that this is what often occurs; explicitly or implicitly, the elements and processes of the model are characterised by attributes which are normative, if they are defined in terms of antecedent psychological conditions. If they are so characterised, there would seem to be no objection to talking about physiological processes in the explanation of behaviour, for example, the physiological activity which occurs when a certain figure is perceived. If they are not so characterised, they are not identifiable except *as* physiological processes occurring at such-and-such a time and place, and they cannot be defined or identified in terms of psychological criteria; if they cannot be defined or identified in this way then it will not be possible to relate them adequately to antecedent psychological conditions. It follows then, that any attempt at explanation in terms of a strictly mechanical model cannot be an explanation which involves reference to antecedent psychological conditions. Thus we have two possibilities. Either the inventor gives strictly physical or mechanical definitions of his model and its inputs, in which

case no statement about behaviour can be deduced, and hence no explanation of behaviour can be given, and in particular no relationships between antecedent psychological conditions and subsequent behaviour can be established; or, alternatively, the elements and processes of the model can be related to, or characterised in terms of, antecedent psychological events or processes, in which case it is not logically impossible to deduce statements of a normative character, about, *e.g.* perceiving or remembering, from the model, since the premisses from which the conclusion is to be deduced already contain statements of a normative sort, but in this case the model is clearly not a mechanical one in the usual sense. If psychologists wish to use models in their attempts to explain behaviour, then it follows from what has been established above that only models of the second sort could be appropriate. Any model of the first sort (strictly mechanical) would necessarily be limited to a model from which statements about behaviour could be *predicted*, rather as the physicist can predict the shape of snowflake crystals from a knowledge of temperatures, humidity, etc., or the physiologist of the future may be able to predict behaviour from a knowledge of what goes on in the body. As an aid to explanation the model would be worthless, and I doubt whether any psychologist is seriously interested in being able to predict behaviour from bodily processes without regard to antecedent psychological conditions, although in one sense of the word such a procedure might establish "causes" of behaviour, in that sufficient conditions for the occurrence of an event had been established.

The fact that psychological words are often used in describing models suggests that most models used by psychologists are of the second sort, although it is often claimed that they are not. In this sort of model the physical or mechanical processes are necessary conditions of the behavioural outcome but not sufficient, since the mechanical processes have to be taken in conjunction with statements, essentially normative in character, about antecedent psychological conditions, whether of memory, perception or knowledge. In conjuction with such statements it has been shown that it is logically possible that statements about behaviour can be deduced from the model (whether this has been successfully accomplished in any known case is another matter) but it would be a mistake to think that therefore an explanation of behaviour had been arrived at different in kind from the sort of explanation in terms of a normative model used by the layman, the philosopher, or indeed the psychologist, when not riding his professional hobby-horse.

The points I have attempted to establish are:

(1) The thesis that behaviour cannot be explained by means of a mechanical or para-mechanical model is unassailable; it rests on a logical point concerning the relationship of entailment. It has been shown that the usefulness of a formal model in scientific explanations is dependent on the fact that statements can be *deduced* from the model which can be interpreted in such a way that they are verifiable by observation, and no such statement about behaviour can be deduced from a strictly mechanical model.

(2) Psychologists have been loth to accept this thesis because they maintain that physical, organic processes are known to affect behaviour, often in a highly predictable way; in some sense they must therefore be contributory "causes" of behaviour.

(3) The lack of understanding between philosophers on the one hand and psychologists on the other has been traced, at least in part, to confusion over the circumstances in which sufficient conditions can be established for predicting an event, and the circumstances in which sufficient conditions for explaining an event are satisfied.

(4) Analysis of these different sets of conditions has shown that mechanical models can never yield explanations of behaviour, although they could, logically, be adequate for predicting behaviour; the sorts of prediction which could be made, however, would necessarily exclude reference to antecedent psychological conditions—a type of prediction which even psychologists would not consider adequate as an explanation of behaviour.

(5) The sorts of models psychologists have mostly used have not been of the strictly mechanical variety, although it is often claimed that they are, and the explanations they yield are not different in kind from other sorts of psychological explanation.

In conclusion, it appears that the search for "causes" of behaviour in bodily processes is doomed to failure, if the criterion of a successful search is that the discovery of such "causes" will force us to recast the conceptual framework of psychological explanation, description and evaluation.

University of London

II.—A WORD FOR DICTIONARIES

By C. Douglas McGee

It is time that someone put in a good word for dictionaries. I should like to call attention to the homely fact that dictionaries are used as rule-books by the users of natural or ordinary language.[1] From this fact, from this normative use of dictionary definitions, can be derived a specific and limited sense for 'true by (dictionary) definition', or 'synonymous or analytic in a natural language'—but this sense turns out to be somewhat peculiar. Also this regulative use of dictionaries has certain analogies to the role of statute law in governing non-linguistic behaviour—but, as analogies will, it turns out to fail us just when we may feel that we need it most. To insist on the fact of this use of dictionaries, and to follow for a way these analogies, these are the purposes of the present essay.

I. *The Normative Use of Dictionary Definitions.* The resembling behaviour of the members of a group, including their linguistic behaviour, is perhaps most naturally explained by reference to the sorts of conditioning they have shared; the existence of common understanding is a function of deeper similarities, reflecting as it does experiences, interests and training which for any individual are much like those of the other individuals with whom he is most closely or most usually associated. In whatever degree this latter community is deficient, that far will common understanding be difficult to achieve. To this truism must be added another: if there is anything as striking as the monotonous sameness of the human situation, it is its infinite variety. No one's training, linguistic or otherwise, is exactly like another person's, to say nothing of that of an entire group. To count on no more than similarity of conditioning, even when the similarity is great, is often to risk failure of common understanding; indeed, one's own training in the use of language is not at any time a finished thing. It is these facts which give rise to what can be called 'the normative use of dictionary definitions'.

No doubt it is obvious that our linguistic habits are the never-finished product of a complex conditioning: we learn by unconscious assimilation, by imitation, by the influence of unchosen

[1] *Cf.* M. G. White, "The analytic and the synthetic: an untenable dualism", in *John Dewey* (S. Hook, ed.) (N.Y.: Dial, 1950), pp. 316-330, esp. pp. 321-323.

and chosen models, by trial and error, praise and blame, criticism and correction. It is also worth remembering—and I am indebted to Professor Ryle for reminding me of it—that considerations of correctness and incorrectness are with us from the beginning, and that we are subject to criticism almost from the start. Considerations of correctness are always present: the relatively crude requirements of successful communication are enough to ensure their operation; they will probably be more or less conspicuous depending on the extent to which those about us consider as important demands of linguistic propriety which exceed the relatively crude requirements of successful communication. Some of us—though probably very few readers of MIND—have learned our language largely through a comparatively simple kind of imitation: amongst us folk it don't hardly matter how you all talk nohow; you put over jist about what your tryin' to git acrost jist so long as you put the right names onto things. Others of us can say—and mean it almost literally—that our linguistic habits have been whipped into shape. Most of us are between these extremes, but all of us are in one way or another highly conditioned. The very effectiveness of this conditioning in the use of language raises a problem of philosophical propriety: the result of our haphazard and deliberate learning, of trial and error, commendation and correction, is the formation of habitual or dispositional patterns of expression and response, of relatively unconsidered regularities in linguistic behaviour; that is the result at which the training aims, when it aims. But despite the considerations of correctness that presided over their birth, and despite the fact that among the characteristic manifestations of such dispositional patterns are feelings of legitimacy or impropriety *vis-à-vis* some uses of language, the attempt to name such regularities 'linguistic *rules*' will encounter some well-known objections.[1] Whether or not these Quineine objections are as well-taken as they are well-known, it would seem safer to reserve 'rule' for something explicitly written down, and which can thus be explicitly looked up. If we are thus cautious in our use of 'rule'—and I shall try to be—then in the course of our discussion we shall have to show the compatibility of these two points: (*a*) That it is relations of predominantly causal, inexplicit patterns of linguistic habit which represent the only relations of 'synonymy *implicit* in

[1] *Cf.* W. V. Quine, "Notes on existence and necessity", *Journal of Philosophy*, 40 (1943), pp. 113-127; W. V. Quine, "Truth by Convention", reprinted in *Readings in Philosophical Analysis* (Feigl and Sellars, eds.) (N.Y.: Appleton-Century-Crofts, 1949), pp. 250-273, esp. pp. 272-273.

usage ', and thus that it is the manifestations of such patterns which the lexicographer ' reports ', and (*b*) That the lexicographer's explicit definitions are nonetheless given a *use* to which 'linguistic rule ' appropriately applies.

It is true that dictionary definitions are empirical reports of linguistic usage :

> The lexicographer is an empirical scientist, whose business is the recording of antecedent facts ; and if he glosses ' bachelor ' as ' unmarried man ' it is because of his belief that there is a relation of synonymy between those forms, implicit in general or preferred usage prior to his own work. The notion of synonymy presupposed here has still to be clarified, presumably in terms relating to linguistic behaviour.[1]

And yet, if this were the whole truth about dictionary definitions, one should expect to encounter dictionaries only in libraries of linguistic research, or at least less frequently beside the family Bible in ordinary but well-regulated households. Even if dictionary definitions are intended by their makers as no more than reports, they are used as norms. As a fact, dictionaries do not attempt indiscriminately to report any and all uses of a term—they could not succeed in this without becoming specifically statistical compilations of enormous complexity. However statistical be the lexicographer's methods of investigation, those *results* of his labour which actually appear as explicit definitions, and which are thus consulted by those who use his book, embody the standard, or at least a standardized version of the usage of a linguistic community.

A. *The Codification of Custom.* Dictionary definitions can be used as rules because they are not merely reports. Even if we eliminate all question of ' good ' or ' preferred ' usage, and suppose that dictionaries report some standard usage which has been determined in the completely democratic manner of a Gallup poll, that is, if we suppose the only ' norm ' involved to be statistical, it is still true that the relation of terms or meanings set down in the dictionary will represent an idealization in respect to the actual usage of any particular speaker of the language, will stand to the individual user and his use less as report than as paradigm. Dictionaries report the usage of a whole group of people, and while the group is made up of Messrs. X, Y, and Z, it may and frequently does happen that the standardization to be found in the dictionary is not exactly equivalent to the actual usage of Mr. X, nor to that of Mr. Y, nor to that of Mr. Z. This

[1] W. V. Quine, *From a Logical Point of View* (Cambridge : Harvard University Press, 1953), p. 24.

is one reason why these gentlemen all own dictionaries. Often enough Mr. Z could for communication safely rely on that uncodified common understanding—those resembling dispositional patterns of meaning and response in which X, Y, and Z have all been trained—which the dictionary has attempted to report, but Mr. Z knows that he will be safer yet if he deliberately conforms his usage to that ideal standard usage which the dictionary maker has distilled, and which is codified in his work. This will be the more so if he can persuade or compel X and Y to do the same.

There is a further reason why the user of a natural language may take his dictionary as a book of rules: Z often finds that the standard equivalences which the dictionary carves out are sharper and clearer than his own uses. The very fact that dictionaries are explicit lists of explicit relations of meaning gives them an advantage in clarity and precision over the actual uses of most speakers of the language. For this reason, a dictionary would be of use even to the solitary inhabitant of a desert island, supposing that he wanted to achieve clarity and consistency in his soliloquies. Its status as report and its accuracy as report would, of course, remain unaffected by the castaway's use of it: like other empirical reports, his dictionary definitions would enable him to predict the behaviour of his linguistic group, in the happy event that some members of it should appear. But this function of his lexicon, and indeed its accuracy as report, would be far less important to him than such help as it gave in ordering his verbal discourse with himself. One might go so far as to say that for the perpetual desert islander, the truth of his dictionary reports would be altogether irrelevant; in his use of them they would be purely regulative, descriptive of nothing, and hence true of nothing. That odd duality of dictionary definitions as reports *and* rules would in this extreme case be insignificant; but both of these dimensions are operative in less solitary, more communal contexts, where dictionary definitions serve as reports and predictions and rules.

1. *The Principle of Petrified Prediction.* Dictionaries thus represent codifications of custom in two reciprocally related but not identical ways: they serve mutually to stabilize and to regulate the linguistic practices of the group whose linguistic practices they report, and they are used by the individual members of that group as rules in the clarification and organization of the member's individual uses. We shall see in a moment that it may be difficult to discern and to discriminate these regulative functions

in the actual context of their use; here I would briefly consider the latter in partial and somewhat artificial isolation.

As an earlier Lewis might have said, in a more rhetorical version of *Mind and the World-Order*, definitions are petrified predictions. If characteristics 'ψ' are singled out as the nominal essence, or essential meaning, or sense meaning of term "Q"—as the criterion for the application of "Q"—that is because the incidence of that particular characteristic or configuration of characteristics has been learned as the best available symptom of the presence of that wider pattern of characteristics whose underdetermined bourne includes whatever Z expects of those things he calls "Q". The recognition of ψ in the passing flux of experience (the taking of some character of a presented situation as similar to, or even indistinguishable from ψ as criterion determined in advance of that situation) is thus not merely the occasion and cue for the application of "Q", but the application of "Q" is in itself a classification, and a condensed prediction. Exactly how far the explicit determination of what shall be essential to the applicability of "Q" need go, exactly how rigid the determination of criteria in advance of application must be before it will be proper to speak of 'definition' may be a point of controversy; in any case the relations of terms set down in the dictionary appear to have an accepted claim to this title.

When a pattern of meaning which has been more or less fixed in its common use is further frozen into an explicit dictionary definition, then at least it is possible for Z to refer to it as a general rule, governing his particular uses of the language. What had been the individual's conditioned determination to classify, and to apply and to substitute terms in such and such a way, can be replaced by his acceptance of an authoritative and extra-personal standard: an explicit rule for classification and for verbal exchange. In so far as a dictionary definition is so taken, so far as it is *used as a general rule*, that far do both of its referential functions become, so to say, logically irrelevant. It does refer to customary linguistic practice, and consequently it does predict linguistic behaviour, but this useful reference of it is neither accepted nor denied by the acceptance of the definition as a linguistic rule. Likewise that function of it as reliable prediction of some kind of experience, which will ordinarily have been its genesis, and will still furnish one reason why it is used, will not be the same as that use to which it is put, when the definition is taken as rule. As a report of ordinary usage it will presumably be true, but no one would think of supposing it necessarily true. In its function as prediction it will have been true in general,

that is, the particular expectations which were its manifestations will have been regularly fulfilled, and this will continue to be the case. But so far as that codification or those standard equivalences set down in the dictionary are accepted and used as linguistic regulations, they are neither probably true nor necessarily true; as *rules* they are not true at all.

2. *The Differing Referents of "Necessary" and "True".* The attribution of 'necessary truth' to dictionary definitions themselves represents an unlikely claim: if a dictionary definition *qua* rule is not true, and *qua* report or prediction is not necessarily true, it will not do to call dictionary definitions necessarily true. But what of a sentence made by substituting, in explicit accordance with, or in subjection to the governance of a dictionary definition taken as a rule for substitution, D-synonyms (that is, terms which according to the dictionary are alike in meaning) for their equivalents in a logical truth? Will not the resultant sentence be necessarily true?

To understand what might with some justice be called 'necessary' and what should not be so called in a sentence thus 'true by dictionary definition', we must try to make a distinction between the acceptance of a general rule, and the assertion of particular sentences formulated in accordance with it. It may be noted that the ordinary user of ordinary language, Mr. Z, is in practice likely to ignore this distinction between the acceptance of a rule and the assertion of a truth and yet to be understood by X and Y to have justified his claim that a sentence is 'true by definition' simply by showing them the relevant definitions in the dictionary. If, however, Z claims that some particular sentence which is thus accepted as true by definition could not but be true, or is necessarily true, the argument may shift its ground so that this distinction—between the particular sentence which is itself true, but not in itself assertable *a priori*, and the rule for its formation which is not true, but has a kind of necessity—may emerge even in the course of almost ordinary conversation.

For example (a slightly recondite example, but too familiar terms might blunt the point), suppose Mr. Z claims that the sentence "All zygospores are formed by the conjunction of two gametes" is true by definition. (Perhaps we users of philosophical language might agree to call such a sentence 'D-analytic'.) Ordinary Mr. X consults his dictionary and agrees, but adds that some zygotes consist of only one cell. "No", says Z, "that cannot be!", and he shows X that, on the same page of the dictionary, "zygospore" and "zygote" are defined

as (D) synonymous, and that "gamete" is elsewhere defined as "A matured sex cell or germ cell, etc." "Therefore", says Z, "you will agree that 'All zygotes consist of two cells' is likewise true by definition?" X reluctantly assents. "Then you must acknowledge", cries Z, "that 'all zygotes consist of two cells' is *necessarily true*!" "No!", replies X, to Z's discomfiture. "I acknowledge the fact that all the zygotes heretofore observed have consisted of two cells, but you do not know *a priori* that some zygote with a solitary cell will not be discovered tomorrow. And even if all the zygotes that are now, have been, or ever shall be, consist of two cells, that does not suffice to prove that a single-celled zygote is an impossibility, but only that nature in fact never produces one."

Z may at this juncture insist that X has missed his point, particularly in X's first accusation: Z will claim that he knows *a priori* the sort of thing the dictionary permits him to *call* "zygote". But this qualification will have the effect of drawing a distinction which Z may wish to regard as a partial concession, at least if Z can see in X's last statement an adequate explication of the *truth* of "All zygotes have two cells". Z may wish to acknowledge that X's description of zygotes under the aspect of eternity—that all zygotes past, present and future have two cells—states all that could be claimed (and names all that would be named) in claiming "All zygotes have two cells" to be true. He may even concede that nothing is added to this truth of the statement by calling it 'necessary'—whatever is in this sense 'true' is necessarily true—without conceding that 'necessary' signalizes nothing whatever. What Z is likely to realize as a result of these admissions is that the great assurance he still feels in asserting "All zygotes have two cells", and which he celebrates by calling the sentence 'necessarily true', cannot cogently pertain to the ways in which gametes will in fact behave in the infinite or unknown future. Only The Omniscient knows the whole of the past, present and future, and what is in this sense 'true'. Even so, Z may insist that his assurance is neither presumptuous nor altogether unwarranted, but has only been misplaced. In so far as it is an assurance which goes beyond a probable knowledge of the future behaviour of zygotes, just that far does it pertain less to their conduct, and more to his own. Z at this moment, pretending not to a tenseless insight into past, present and future, but having looked into the dictionary, has decided to accept its definition as a general rule, and has accordingly determined never to call anything "zygote" unless he also is willing to say "this has been formed by the conjunction of two

gametes". He takes this resolution and declares it to be binding on all his future use, but it is accomplished at the present moment: he has decided, in advance of the future, and at least in this sense *a priori*, the criteria that will govern his particular uses of these terms; if he failed altogether to fix his intentions, not only would he be unable unambiguously to apply "zygote" and "zygospore" to any particular gathering of gametes that at some particular time appeared, but even supposing all existence spread out at once before him, on the Last Day, so to say, he would not be able clearly to separate even the sheep from the zygotes.

Obviously there is no claim here that any relation of meanings is inherently or intrinsically necessary. Dictionary definitions can be and are *used* as explicit and general rules: their specification of a nominal essence for a term can be taken as a rule for the application of that term, and for its exchange or substitution for other terms for which a like nominal essence is specified. But nothing is named by a rule, not even the particular uses it governs; it cannot be necessarily true because it cannot be true. Those particular sentences which are formulated according to its governance, and which at the same time purport to name an actual state of affairs, or to describe a relation among matters of fact, will ordinarily be true, but nothing is added to this truth of them by calling it 'necessary'—unless it is the very doubtful claim that one has a kind of empirical knowledge which transcends human limitations and is cogently characterized as 'certain'. If what is meant by calling such a sentence 'necessarily true' is that its *assertion* will be maintained under any factual circumstances whatever, that it will never be withdrawn, come what experiences may, then the 'necessity' of the sentence will not be a function of the facts or the experiences, but of the determination of its asserter so to assert it. Since *any* sentence can be so asserted—given sufficient stubbornness, and a complete refusal to consider the independent question of utility—it is to the justification of such assertion rather than to the sentences asserted that one must look to discover and assess any validity of their necessity. But this is to reintroduce the reference to rules: one usual and generally accepted way of justifying the 'necessity' of such assertion is to show that the assertion has been made in accordance with a specified scheme of classification; one usual and generally accepted scheme of classification is found fixed in dictionary definitions. One in this way justifies the necessity of his assertion by showing that this assertion conforms to a particular relation of criteria, and justifies his commitment to that relation of criteria by showing that relation

to be codified in a dictionary definition; one in this way declares a general resolution (more or less 'necessarily' or absolutely) binding on his linguistic behaviour, and justified by reference to the dictionary as a book of rules.

It is not unlikely that the justification of *this* justification will be solidly pragmatic—based on the utilitarian virtues of the dictionary's particular fixations of criteria, or on the desirability of regulated communication—but such assertions of truth as are advanced on this level of justification will be qualified as probable. If it should seem from this that when 'truth' comes in the door, 'necessity' flies out the window, and *vice versa*, perhaps this is an indication that 'necessary truth' is in this context a peculiar or unstable compound, being either redundant, or trying simultaneously to name what is regulative and what is referential in the use of language.

B. *From Petrification to Prescription.* A dictionary definition has in its regulative use such 'necessity' as characterizes a self-imposed rule; its utility is a different matter. Z's relation to dictionary definitions is more complex than that of a desert islander: the latter is more autonomous in his acceptance of regulation, since the particular utilitarian demands of communication do not exist for him and, for use in classification and soliloquizing, he could make up his own rules, and even write them down if he wanted to.[1] Z on the other hand is not so free, and his acceptance of a rule will be in psychological fact all but indistinguishable from his belief in its utility for inter-communication, or his fear that non-conformity will be followed by failures in mutual understanding. The circumstances which keep Z from trespassing against dictionary definitions are not unlike those which restrain the ordinary citizen from shooting at his neighbours: the latter has decided for himself, though after considerable conditioning, that this is the wrong sort of thing to do, that he will not shoot his neighbour however great the temptation in particular cases, and he has taken this as a general rule for his own conduct; but he also knows that it is against the law, and the fear of being caught and punished contributes its beneficent influence in an inextricable fashion.

But sometimes ordinary citizens *do* shoot their neighbours, and take their chances on the consequences. And sometimes Z does ignore or violate even those rules which from the dictionary he has taken for his own, for the better ordering of his linguistic

[1] Even Mr. Z often does something very like this: his practice gives rise to what can be called '*ad hoc* linguistic rules'—that is an interesting but slightly different story.

behaviour. No such rule compels its own acceptance: the explanation of its genesis (in terms of the growth of dispositional patterns of expectation and prediction, and their final freezing into definitions) or the justification of its utility (in terms of its value for classification and communication) are neither one sufficient to account for that force it has so long as it is accepted as a rule which the individual takes, *a priori*, as absolutely binding on his use of the governed terms; but nothing about the rule itself is sufficient to assure that it will be so accepted. Those equivocations, intentional or merely careless, those shifts in meaning, deliberate or unobserved, those refusals or withdrawals of "Q" even when its application is required by determined criteria for its application or substitution—such loose practice, such indeterminateness in the specification and application of criteria tends to be lessened by any possibility of correction, but especially by the possibility of recourse to an explicit, relatively invariant and publicly available standard. Conversely, the permanent possibility of such public justification for the attribution of '(D) synonymous' or '(D) analytic' tends to fix the determination of the user in clear and all but infrangible patterns. *All but* infrangible, for even under these comparatively settled and conventionalized circumstances, such determination may be but seldom absolute. It is not just that one carelessly makes particular slips, which are easily and thoroughly corrected by another look in the book; the more troublesome possibility is that the determination of one's own resolution may be imperfect, or his commitment to it incomplete, or both. Then it is that Z may claim the justification of a rule, and yet in individual assertions ignorantly ignore or unscrupulously evade its restrictions. One must note with regret that such slippery practices are not made impossible by the availability of rules—they are only made easier to detect.

What tends to keep Z in willing and consistent *subjection* to dictionary rules is less any intrinsically *a priori* nature of them than it is the extrinsic and practical demands for clear and consistent linguistic behaviour which arise most obviously in the context of a linguistic community (but which are also present if one is a critical listener when talking to oneself). It is not to be expected that some actual pendulum, ticking back and forth behind its actual and grimy glass, would think to itself that it must look up the behaviour of ideal pendulums, and try to regulate itself accordingly. But when Mr. Z looks something up in the dictionary, he takes what he reads as that to which he ought to conform his own linguistic habits, and his particular

uses. Without even trying to cope with the question of determining 'good' usage, we can see how this use of dictionaries sets them up as norms. Mr. Noah Webster and Mr. Shorter Oxford set out to report the linguistic behaviour of a group of people, to describe how they do use words, but when Mr. Y and Mr. Z look into their dictionaries it is more often than not to determine how they *ought* to use words. Since the other dictionary users in the group do the same, the use of Webster's work makes it arbiter to the speech community.[1]

"Arbiter" here might be thought to mean either "*arbiter elegantiarum*" or "judge". Dictionary definitions as preferred usage may have the former function, but it is more readily instructive to take the latter as an analogy, since dictionary definitions can have in their use certain interesting similarities to and dissimilarities from statute law, when the latter is considered on a simplified Holmesean model, as having dimensions of description and prescription and prediction.[2] Dictionary definitions not only report the habits of a speech community, but by their very explicitness, by their public availability, and by the uses to which they are put, they serve to codify the usage which they report. Before dictionaries, we might extravagantly say, the tribe had customs, and after dictionaries it has laws; what were and remain learned responses, causal regularities—including feelings of propriety and rage—and ancestral ways, are also formalized as explicit statutes. If some linguistic Solon tries to promulgate a fiat which has no relation to customary behaviour, then, though his law may for a time remain 'on the books', it will be ignored or flaunted until it is withdrawn. (No definition without representation!) Conversely, if the citizens of the linguistic commonwealth are persistent in the consistency of their behaviour, then no matter how conservative the lexicographical legislator and despite the less than perfect democracy of the realm, the public use will have its way, and will in time be embodied in the law. The settlement of disputes over what is right to mean, both in the sense of application and of substitution, of classification and consistency, and the adjudication of questions

[1] It is said that Mrs. Webster upon once unexpectedly entering her husband's study discovered the lexicographer with his secretary on his knee. "Mr. Webster", said the indignant wife, "I am surprised!" "No, madam", replied the always correct Mr. Webster, "*You* are astonished. *I* am surprised."

[2] Kant would say that 'laws' in this sense were hypothetical, not categorical imperatives. If the latter enjoin right conduct though the heavens fall, the law inhibits wrong conduct with the prediction of prison, or other dire consequence.

of likeness of meaning can thus and then be accomplished according to the book, and with a properly explicit respect for the linguistic law.

II. *When is A Rule?* While the legal analogy is no better than most, the point of its failure is not without interest. It begins to break down over a difficulty similar to one raised by White, but more particular and consequently less poignant. White contrasts artificial with natural languages, and argues that

> . . . natural languages have no rule-books and the question of whether a given statement is analytic in them is much more difficult. We [*sic*] know that dictionaries are not very helpful in this matter. What some philosophers do is to pretend that natural languages are really quite like these artificial languages; and that even though there is no rule-book for them, people do behave *as if* there were such a book. . . . I suppose that it would be granted that those who use natural language do not make conventions and rules of definition by making a linguistic contract at the dawn of history. What defenders of the view I am criticizing want to hold, however, is that there are other ways of finding out whether a group of people has a convention. And what I am saying is that philosophers should tell us what these ways are before they dub statements in natural languages 'analytic' and 'synthetic'.[1]

Plainly Messrs. X, Y, and Z, the users of natural language L, are not the 'we' who know that dictionaries are not helpful in answering the question whether a given statement in L is D-analytic, and the recognition that dictionaries are in fact used as rule-books tends to lessen the general difficulty of the 'as if', leaving a more particular problem. This is not to defend the view that White is criticizing in the excerpted passages, but to raise a point of fact which he seems by implication to deny: developing through the continual and reciprocal influence of uncodified usage, its report in dictionary definitions, the codifying effect of such reports, their use as rules, and the influence of this use again on less formal fixations of meanings, dictionaries *do* tend in use to have the status and force of a 'linguistic contract', embodying 'conventions and rules of definition'.

The problem that remains is far less a problem for Mr. X who uses language L than it is for Professor A who is concerned—from the outside, so to speak—to analyze L: even if Professor A is willing to allow that dictionaries are often used as the rule-books of this natural language, he may point out the difficulty of discovering just when they are so used. If for his purposes of analysis A demands to know not only how in principle a dictionary

[1] White, *ibid.* pp. 321, 323.

is used as a source of rules, but requires also a sure way of finding out just which particular attributions of 'analytic' in L are those in which the dictionary is cited as a justifying rule, he will point out some serious flaws in the legal analogy. In the first place, the recognition of that multidimensional descriptive-predictive-prescriptive character of statute law which is important for its understanding and interpretation does not make it difficult to identify the membership of *lex scripta*. To discover what is the law, in this sense, one can look in a book marked "Laws": if it is written in that book, it is a law, even if it fail as description and be neglected in practice. While a statute is on the books, that harmony with custom which may have been responsible for its creation, that accuracy as description lacking which it will probably be neglected or not enforced, these questions are irrelevant to its legal status; its identification as *lex scripta* does not require consideration of the use that is made of it. (The question whether it should be kept on the books is of course another matter.)

The same should probably not be said of dictionary definitions: even if we as Professor A decide to count as instances of justification by definition only cases wherein the authority of a dictionary is explicitly claimed, seeking thus to separate behaviour that is merely habitual or customary from conduct in deliberate conformity to a rule, we shall find this restriction is not sufficient to make the required distinction. For as it stands in its book, and in many of its uses, a dictionary definition is a report of usage—standardized and idealized *vis-à-vis* the uses of the individual, but a report for all that. The recognition of the explicit equivalences set down in dictionary definitions as explicit *rules* would seem to require recognition of their *use* as such: *scripta* they will be in any case; it is in their use deliberately to regulate linguistic behaviour that they are *lex*. And how are we to recognize that use as just that use?

We have already seen that the ordinary Mr. Z in his justificatory use of dictionary definitions often fails to distinguish substantive assertions from procedural declarations. This may or may not confuse Z's practice, but it does make A's analysis of it difficult. That referential dimension of definitions which is here most likely to be confounded with their regulative use is their status as reports of customary usage. Suppose that Z is asked whether Ab necessarily precedes Elul, or whether he could consistently assert "Some bongos are bipeds", or whether "*crux ansata*" is synonymous with "ankh"; suppose that having looked into his dictionary, Z says "yes", "no", and "yes". How now

are 'we', as A, to decide whether Z is accepting what he reads as no more than a true albeit standardized report of the ways in which other speakers of his language use the relevant terms—the sorts of things to which they habitually apply the terms, and the substitution of expressions they customarily make—and is passing this report on to us, or whether he has also determined to submit his usage to the governance of standard usage, taken as a rule? The simplest course might be to ask him, but Z might himself find it difficult to make this distinction—there is so seldom pressing need for Z to differentiate between his acceptance of the definition *qua* report of standard usage, and his acceptance of it *qua* standard for his own use. The desert islander who is not interested in communication and to whom the report-status of definitions is unimportant, would perhaps find it less difficult to make this distinction explicit; given Z's greater interest in communication, his acceptance of the definition as rule might only with great care be distinguishable from his belief in the utility of conformity, and this in turn from his belief that the definition accurately reports standard usage. Unlike the law, a dictionary definition has no independent (linguistically) legal status; it is a 'rule of language' only when it is used as such, and the very reasons why it is so used make it difficult to distinguish this primarily regulative function of it.

The analogy between statute law and the regulative use of dictionary definitions fails in a closely related way when we reflect that the latter government knows no separation of powers. The law of the land (*a*) requires that you pay your taxes and that you refrain from shooting your fellow citizens, (*b*) stipulates that the penalty for violation ranges from such and such to so and so, and (*c*) predicts, in effect, that if you do not behave as it requires, the courts will impose the penalties it stipulates. But even when a dictionary definition is used as a rule, it makes no stipulation such as "b", nor can one find in the dictionary any prediction such as "c". This is not to say that the consequences of transgression cannot be predicted, but rather that such prediction, not being explicit in the 'law', might not admit of clear distinction from predictions of habitual (though regular) reactions, or of customary (though unregulated) behaviour. If one were willing to speak of 'implicit' penalties and predictions, he might read into the dictionary's regulative use "Obey these rules of standard usage, or suffer the consequence of not being understood". The ostracism of non-communication with the members of a linguistic group, that Coventry to which is sent the person who does not or will not follow the speech community in meaning

by "spinster" the same as "unmarried woman", an isolation thorough in proportion to the number of such 'rules' transgressed, this could be considered the penalty which follows trespass against linguistic standards. Perhaps the plea of extenuating circumstances serves generally to soften such sanctions: "Obey these rules, or suffer the expense of further explanation; if you do not mean by 'spinster' the same as 'unmarried woman', as that equivalence is codified in the dictionary, then what you say will not be understood by the community unless and until you pay in terms of explicit clarification, or by cashing appropriate and valid verbal letters of exchange."

The analogy here is apt as far as it goes—this *is* the sort of thing that happens to one who does not use words as his neighbours do—but it breaks down when we reflect that since there is no 'penalty' explicitly stated in the 'laws', the only 'penalty' that can be discovered is the actual conduct of the group. If we were to say "The penalty is enforced by the whole community", we should have to acknowledge that since the 'penalty' is just the failure of communication between the miscreant and that community, it would be difficult to distinguish this purportedly punitive situation from one in which there was no breach of *rules*, perhaps because there were no rules, but only a failure of the offender to conform to the uncodified linguistic habits and customs of the community. Since we would avoid reference to a linguistic *lex non scripta*—the notion that dispositional patterns of classification and expression are implicit *rules*—we, as A, are again faced with the difficulty of distinguishing behaviour which we are able only to call 'conditioned' from behaviour which is deliberately regulated.

But is this problem really different from that mentioned three paragraphs back? Let us say that as A we decide to attribute 'breach of rule' or 'use of rule' only to cases where the authority of the dictionary is explicitly invoked in correcting or admonishing some erring Z—as in fact it often is.[1] But now, since the force of the rule is simply that of a hypothetical imperative, how shall we decide whether the 'authority' involved is no more than that of a prediction that, if Z does not change his ways to accord with the linguistic mores of the group, he will not be understood, or

[1] Of course, much or most criticism does not cite the dictionary in justification: either it does not bother to give any justification at all, or it justifies itself by reference to a 'sense of language' or to an unwritten knowledge of correct usage. All of these ways of correction are practised; it happens that we are here primarily concerned with that late development, the use of the dictionary as an explicit and extra-personal authority.

whether the definition is cited as a rule to be followed, the penalty for non-conformity being just that failure of communication which the prediction predicts ?[1] Whereas before we had to ask whether Z accepted the definition as report, or as rule, or as both, now we seem to be asking whether his tutor, Mr. X, say, is *citing* the definition as report, or as rule, or as both. When X demands that his chauffeur drive more slowly, he may cite the statistics on automobile accidents caused by speeding, or he may cite the traffic regulations (those designed to reduce the number of accidents caused by speeding by fixing maximum speeds and stipulating the penalties to which the violater is liable), or he may invoke both. Despite the intimacy of their relation, and the probable difficulty of disentangling their complementary influence on the chauffeur, it is not a formidable task to distinguish the causal from the legal citation : one can refer to different books and threaten different consequences.

In the normative or regulative use of dictionary definitions there is but one book : dictionary definitions can be thought of as analogous to statute law in having both descriptive and regulative dimensions, but they are unlike the law in that with respect to definitions the latter dimension depends for its existence on its use. If ' we ' as A have not yet discovered a clear symptom of the incidence of the primarily regulative use of a dictionary definition, or developed a criterion which will allow us to apply an appropriate analytical or philosophical sense of ' rule-regulated synonymy ' to this class of attributions of likeness of meaning in a natural language, that failure can scarcely stand as evidence against the existence of such use, nor for that matter will it deter Z from looking in the dictionary to determine whether " spinster " is D-synonymous with " unmarried woman ".[2]

If the problem that remains for Professor A—the problem of developing criteria for *his* application of ' D-synonymous ' or ' D-analytic ', criteria which would allow the analyst of ordinary language unambiguously to apply his meta-linguistic labels to particular instances of linguistic practice—if this is a problem worth considering, it would seem not to be a problem purely philosophical, or exclusively analytic. Perhaps it is at such

[1] The desert islander has a greater autonomy than Z, his rules being more freely self-imposed, but even for him the imperative is hypothetical : follow these rules if you would be clear in communing with yourself. The desert islander obviously could be indifferent to this consideration, just as it is possible for Z to be indifferent to the demands of successful communication.

[2] *Cf. Webster's Collegiate Dictionary* (Springfield, Mass. : Merriam, 1943), p. 959. The answer would appear to be " yes ".

points that one might hope for a cautious merging of responsibilities between the analytic philosopher who really was interested in ordinary language, and the empirical investigator of linguistic practice. If, as will often be the case, a philosopher is not really interested in ordinary language at all, but only—as a popular and misleading gesture—says that he is, if he is really concerned to develop an extraordinary and esoterically philosophical language designed according to peculiarly philosophical criteria in order to meet peculiarly philosophical needs, then he may well find himself dissatisfied not only with X, Y, and Z's so serviceable 'D-analyticity' but dissatisfied *a priori*, so to speak, with any analogous determination which ordinary language *could* fix. The philosopher who has thus decided this question in advance might save confusion all around if he took the first convenient opportunity to declare himself out of the ordinary language business.

Vassar College, New York

III.—ON THE LOGICAL INDETERMINACY OF A FREE CHOICE

By D. M. MacKay

1. *Introduction*

Nobody knows for certain the extent to which the much-discussed "iron chain of cause and effect" operates in the mechanism of the brain. Those of us whose task is to gather experimental evidence on the question find discussion frequently obscured by emotional prejudice based on the idea that a completely causal explanation, in physical terms, of brain-function would make what Phillip Frank[1] calls "the firm subjective conviction that we have a free will" an illusion.

I will not here discuss the attempts which have been made to invoke Heisenberg's Principle of Indeterminacy as a way of escape. I have argued elsewhere[2] that although physical indeterminacy might indeed give cerebral activity the spontaneity and originality which is associated with one aspect of "free-will", we would be misguided and immoral to regard such indeterminacy as essential to the 'freedom' about which we have this 'firm subjective conviction'.

Nor do I wish to debate directly with those (of whom Professor Frank appears to be one) who despair of the genuineness of such freedom and seek to find room for morality without it. I want rather to attack the alleged ground of their despair, without which the whole pressure behind such arguments disappears. I believe it rests on a presupposition which is widely accepted, but which may nevertheless be questioned for reasons which have nothing to do with physical indeterminacy.

The usual form of argument is as follows: if my physical brain-processes were wholly physically determined, and if my decisions could be inferred uniquely from my brain-processes, then a fully-informed observer of my brain-processes could know the outcome of my choices with certainty before I made them, and my impression of freedom in making these choices would therefore be an illusion, due to mere ignorance of the true state of affairs.

I believe that a close scrutiny of the way in which this argument breaks down may throw some useful light on the whole problem of relating 'mental' and 'physical' talk about human beings.

Our discussion of it falls into three stages. First, we shall take note of some peculiar restrictions on the predictive certainty

attainable in principle by the observer of an agent. In the light of these, we must next question whether 'the true state of affairs' can validly be identified with the view of the observer, and whether the agent who does not share that view can validly be described as 'ignorant'. The answer will lead us finally to recognise a curious kind of 'relativity principle' governing talk about the acts of agents, which—if I am right—shows the stock argument above to be fallacious. All this, be it noted, will involve us in no necessity to deny (or affirm) the physically-determinate character of brain processes.

2. *The need for secrecy*

At first sight it may seem inevitable commonsense that if everyone else in the world could be rightly certain that my brain would go through a particular sequence, I should be worse than deluded if I held a different view. If I were to refuse to accept publicly verified evidence that I have red hair, or that I have no back teeth, the commonsense conclusion would indeed seem to be justified. Why then should statements about my brain be in a different category?

The first clue to a possible answer comes when we reflect that whereas my hair or my teeth are substantially unaffected if someone tells me what he believes about them, my brain, as the organ of understanding, is in a fundamentally different situation.[2,3] Anyone who wished to make a reliable and complete prediction of my brain-activity might in fact have to take great pains to *prevent* my coming to know of it, or even coming under the influence of any relevant factors determined by the conclusion he reaches. The reason is not primarily psychological but logical. His prediction, to be successful, must allow for any relevant effect its formulation and communication will have on my brain; but these effects could not all in general be calculated unless the prediction itself were already known, so that in general the exact calculation can never be completed. This is in fact a similar logical situation to that treated by Popper[4] in a penetrating analysis of the limitations of computing machines, and although the present argument does not depend on the validity of Popper's thesis, it must be admitted that for at least an important class of cerebral states, no one who intended to *tell* me his prediction of my cerebral activity could remain logically certain of its success. On the contrary, I could quite properly, and on excellent logical grounds, defy anyone to tell me with certainty beforehand the outcome of most of my choices, even if the physical processes

in my brain were wholly determinate in the sense of classical physics and fully accessible to his observation, provided only that the information-receiving system of my brain was causally linked in the right way with my choice-mechanism.*

3. *Conditional certainty*

As an answer to the disputed step in the stock argument, however, this is hardly satisfactory. It does indeed show that my brain could be physically determinate in operation, and yet unpredictable in principle to members of a community whose predictions are accessible to me or are determinants of factors affecting my decisions.

This amounts to saying that in such a case no one in fact could be validly certain that my brain would go through a particular sequence. What it shows is not that I can disregard other people's certainty when they are entitled to it, but that in one important case—namely where they wish to tell me, or cannot help affecting me by, their prediction—they could not achieve such certainty, even if the "iron chain of cause and effect" were known to operate.

We are thus still left with our main question: If (by preventing me from knowing or being influenced by their prediction until after the event) everyone else *could* validly be certain how I shall choose, can I justly claim that my choice is still free? Surely, it might be argued, I am simply showing my, perhaps necessary, ignorance of the true state of affairs; I am under an illusion?

A preliminray skirmish might well take place over the concept of certainty as used here. When I am certain that 13 is a prime number, or that London (England) is on the Thames, I feel I am up against facts that everyone must reckon with. I don't care who knows them. But the 'certainty' of a secret prediction P of an agent A's subjectively free choice is subtly different. It is only a conditional certainty—a certainty whose value varies according to the person contemplating the prediction. In short, despite the public character of the proposition P, truth, reliability, or certainty cannot here be attributed to it in isolation, but only to P-in-relation-to-the-person-entertaining-P. It is *people*, not propositions, who are certain, either rightly or wrongly. We cannot automatically attribute certainty even to propositions which they are certainly right to maintain.

Nor do we escape this qualification by modifying the prediction as 'Provided A does not get to hear of this, he is certain to do X';

*But see section 7 below.

for this new proposition in turn will have a different truth-value for A and for the silent onlookers. At most, A could say of it 'This is out of date'. For A, it has no predictive information-content.

There is a further logical qualification of the 'certainty' of the silent onlookers. If we asked them (quietly, so as not to disturb A) 'How can you be certain that A will not be affected by your calculating P?' their answer might have to refer in part to their *intentions*: 'we *intend* not to tell him or influence him'. Their 'certainty' that A will do X is thus conditional on their own fulfilling certain intentions; and (by the same line of argument) they could never in general have all the up-to-date information about their own brain to predict this with even the qualified certainty which they have about A's action. Can they be *certain*, for example, that a wave of pity for A will not sweep over them and impel them to intervene? Presumably their best course would be to destroy all outgoing lines of communication with A, as the practical equivalent of hardening their hearts. From a moral standpoint this action would appear to give them a significant share in responsibility for A's choice, since they are taking deliberate steps to ensure that it shall be X when they know or fear that it might otherwise be not-X. But it would I think remove one element of uncertainty as to their success; and our purpose will be served equally well if we suppose that the outgoing communication channels are cut by some third agency.

4. *Logical indeterminacy*

Assuming then that all this is done, and the onlookers are powerless spectators of a sequence whose outcome, for them, is 'certain'—what can A validly believe about the choice he has yet to make? If he knows nothing of the onlookers and their predictive knowledge, he may ignorantly suppose that no one can tell how he will choose. In this, *ex hypothesi*, he would be simply mistaken, and if his subjective conviction of freedom amounted to no more than this belief, it would indeed be illusory.

Suppose, however, that A knows all about the onlookers, and has sufficient past evidence (say from predictions secretly written down and shown to him after the events they predicted) to convince him that they have these predictive powers, so that he believes they *can* be secretly certain of the outcome of a choice he has yet to make. Can he still validly believe he is choosing freely, not only in the sense of feeling unrestrained, but also in the sense of

having a genuine option logically open to him? Or is he merely ignorant of what the onlookers know to be really the case, and so still under an illusion?

It seems to me that he is not merely entitled to believe that his choice is undetermined, but that this is the only view of it that he may validly hold until he has made it. For him, despite all we have just granted, any proposition purporting to specify his choice in advance is simply *logically indeterminate* until he makes the choice. In other words, I wish to deny, as itself fallacious, the view that his subjective conviction of freedom is based on 'ignorance' of the 'true state of affairs'.

The argument is twofold. First, we must question the identification of the onlookers' view in this case with the 'true state of affairs'; and secondly, we must ask whether A's lack of an onlooker's predictive knowledge of his own brain-workings can properly be described as ignorance. I shall argue (1) that the onlookers, despite their numerical superiority, are here disqualified equally with A from claiming to describe the 'true state of affairs'; and (2) that as the crucial belief on which the onlookers base their prediction has here no predictive information-content for A, it is a misuse of words to describe A's relationship to it as '*ignorance* of the true state of affairs'.

5. *The elusiveness of 'the true view of affairs'*

The first question might seem to strike at the root of our whole idea of objectivity. If the true view of affairs is not the view from Olympus, we may ask, what standard of objective reality is left? I do not propose now to follow this metaphysical sidetrail, fundamental though I believe its goal to be.*

All I wish to do here is to press home the implications of the *necessary secrecy* surrounding the onlookers' knowledge. There is no dispute that *they* are right to believe what they do about A's brain-processes. But even they would insist that *A* would not be right to believe the same, since a precondition of its validity is that A must not be influenced by it. Clearly then the onlookers' view represents a true description of the state of affairs only *for the onlookers*, since if it were universally true, A would be wrong *not* to believe it. From a logical standpoint their numerical superiority is irrelevant; and A need not be terrified at finding

* Let me only point out that the 'view from Olympus'—the view of the non-participant observer, which I am rejecting as an absolute standard, is something very different from, for example, the 'knowledge' attributed to God in Christian theology, which in a special sense combines that of spectator and participant.

himself out of step. There are essentially just two parties in the situation, one looking on and one making a choice. The onlookers' view can logically be recommended as valid only to one of the two.

What then of the other party? A's view is necessarily different from the onlookers'—but it is not arbitrarily different. Indeed there is *ex hypothesi* a definite correspondence between the two, so that the validity of A's description could be checked, afterwards, in terms of the onlookers'. But—and this is the point—the check is not for identity between A's and the onlookers' descriptions; on the contrary, in order that A's should be valid, the onlookers' must not in general say the same thing—and conversely.

Thus on the one hand, the idea that either party can give a universally-valid description of the 'true state of affairs' in this case is false; on the other hand, any idea that this proves there is *no* 'true state of affairs' is invalidated on the assumption that the two descriptions stand in a rigid relationship. We might call them two different but related 'linguistic projections' of one and the same state of affairs. It is perhaps not surprising, if tantalising, that no single standpoint, whether of onlooker or agent, appears to allow us to put into words the whole truth about ourselves.

6. *The 'ignorance' of a free agent*

What then of our second question? Can A be said to be 'ignorant' of a fact that the onlookers 'know for certain'? In one sense it is doubtless true that A does not know what they know: in fact they have taken care that he does not. Normally we can translate 'A does not know what B knows' by saying 'A is ignorant of a fact known to B'. But here we are in trouble, for we have already seen that the onlookers' prediction P does not have a unique factual status in isolation. The interesting point which emerges is that what we are tempted to call A's 'ignorance' *would not be remedied by supplying him with the proposition P describing the state of affairs of which we are trying to say he is ignorant*, since P would lose its factual status if A were to entertain it. In short, the onlookers *have no predictive information to give A*, even if they would. A may not realise this, and may even 'wish he knew' what they know; but in respect of predictive information his wish is based on a fallacy—the fallacy of supposing that what he wants to know is a universal fact. The truth would seem to be that at this point there is no gap in his knowledge;

the place of the onlookers' knowledge is already preoccupied for him by the knowledge that the choice awaits his decision. To make room for it, he would have to resign from his role as agent: but then the choice would not be made.

7. *Criteria of logical indeterminacy*

Obviously not all of the onlookers' knowledge is of the peculiarly evaporative character we have been discussing. All of us constantly gain useful information about ourselves from others which we would find it hard to obtain unaided. We must then try to make explicit the conditions under which propositions are not merely unknown by A but *logically indeterminate* to A.

The general condition is easily established. We have been working out the assumption that activities such as formulating or believing a statement require corresponding brain-processes to take place. Consider now the class of statements that describe the subject's own brain-processes or activities causally linked therewith. Among these, there will be a subclass which cannot be formulated or cannot be believed by the subject, without brain-activity that interferes with the brain-process they describe. It is unnecessary to assume that *all* such interference nullifies the statement.[4,5] One of the best ways of making an angry man more angry may be to tell him he is getting angry! What we can always say is that in such a case the formulating or the believing of the statement becomes one of the factors determining its truth or falsehood, so that it cannot claim unique factual status in isolation. But where the brain-activity in formulating a statement is by itself sufficient to make it false and so out of date, an up-to-date formulation becomes impossible to the subject.

Similarly, where the brain-activity in believing a statement is by itself sufficient to make the statement false (though disbelief might not suffice to make it true), the statement for the subject is *logically incredible* in the strict sense of the term.

My suggestion, then, is that our 'firm subjective conviction of freedom' is not primarily a belief about the unpredictability of our brain-processes but is the entirely justifiable corollary of these peculiar logical facts. For us as agents, any purported prediction of our normal choices as 'certain' is strictly *incredible*, and the key evidence for it *unformulable*. It is not that the evidence is unknown to us; in the nature of the case, no evidence-for-us at that point exists. To us, our choice is logically indeterminate, until we make it. For us, choosing is not something to be observed or predicted, but to be done.

I thus find myself in a curious blend of agreement and disagreement with Professor Ryle, who draws attention to some of the same peculiarities in his analysis of self-prediction in *The Concept of Mind*. On the one hand (p. 197) he agrees that 'the fact that ... my future is systematically elusive to me has ... no tendency to prove that my career is in principle unpredictable to prophets other than myself'. Yet he elsewhere (pp. 77-80) uses analogies of games of chess and billiards, and of the writing of prose, where the point is repeatedly made that from the initial conditions and the rules, the subsequent course cannot be uniquely determined. It is difficult not to construe this passage as a denial of the predictive sufficiency (for the onlooker) of an onlooker's mechanical data—a denial which I would argue to be philosophically needless. My chief disagreement—or perhaps difference of emphasis—however, comes where Ryle speaks of the self-predictor as 'having to overlook at least one of the data' relevant to his prediction (p. 197). It is the presupposition underlying this way of expressing the case that I have been chiefly concerned to question.

As will now be clear, this is the presupposition that the acts of an agent can be characterised, like the workings of any physical object, by a set of data expressible uniquely in propositions universally valid in and mandatory on the linguistic community, so that it always makes sense to speak of the agent in particular as 'overlooking' some or 'ignorant' of others. This goes with a more basic notion that might be called the presupposition of *transferability*: that if A agrees that B is right in believing P, A logically commits himself to P and to all consequences deducible from it. Despite its obvious validity in most contexts, we have seen that this can break down where P is an assertion about an agent as viewed by an observer. If it were logically (as distinct from physically) possible to exclude the agent from membership of the linguistic community, then the presupposition could stand as it does in face of an inanimate agency. If, however, we are concerned to ask what a *member* of our linguistic community may validly believe (*e.g.*) about the actions he would subjectively term 'free', I see no escape from the necessity to make room for two complementary stories, one validly believable only from the standpoint of detached onlookers, which the agent would be wrong to try to believe until after his action,* the other validly

*In retrospect, of course, the agent can join the onlookers (e.g. in witnessing a moving film of his own brain processes) and share in their 'outside' view of his physical past as 'determined'. Past and future have an asymmetric logic for an agent.

believable from the standpoint of the agent,—and by the onlookers too insofar as they permit themselves the exercise of sympathetic imagination. The two are not mere *translations* of one another, since it is *what is asserted* by the onlookers, and not just its conventional *form*, that is logically unacceptable to the agent; yet (*ex hypothesi*) there is a definite *transformation rule* by which (in retrospect at least) the accuracy of one may be checked in terms of the other.

8. *Conclusion*

Implicit in this treatment of a well-worn topic there have been one or two ideas which it might be well to make explicit in conclusion. First, we have found it expedient to shift our emphasis still further than some modern philosophers from discussion of the truth of certain *propositions* in the abstract, to discussion of the validity of the activities of *formulating and believing* these propositions. By doing so we have been able to express in non-contradictory terms what would otherwise have had to be expressed in paradox. It seems possible that this shift of emphasis might be rewarding (and far from destructive) in some other metaphysical and theological contexts. Second, and not unrelated, there has emerged the idea of *transformation rules* according to which it may be essential for A's belief to differ from B's in order that both may be valid. This denial of simple transferability constitutes a kind of philosophical Principle of Relativity, very different from that exaltation of the arbitrary which goes by the name of 'moral relativism'. It resembles rather Einstein's physical principle in its insistence (i) that only one rigorously prescribable belief is valid for A if B's belief is also valid, but (ii) that the validity and meaningfulness of a belief may depend in a definite and rigorous way upon who entertains it. It differs, however, in giving no guarantee that A can even formulate from his standpoint the belief that would be valid for B (until it is out of date) and in making no assumption that their situations must be symmetrical.

Despite its radical consequences this principle has no sanction for the idea of 'two kinds of truth'; and of course it leaves the bulk of public human discourse unaffected. But it does suggest that—and why—the traditional method of *comparing notes* in order to 'arrive at the truth' must break down in certain special cases, leaving the truth in such cases incapable of unique and universally valid expression.

This brings me to a final point. To discover that nothing

physically odd *need* accompany a free action, though we have no way as yet of knowing whether it does, undoubtedly disposes of a potential 'mystery'; but it is far from showing, as Ryle (*loc. cit.* p. 198) avers, that 'there is nothing mysterious' about our self-consciousness. On the contrary, I suggest that it only helps the really mysterious aspect of selfhood to be more squarely faced: for to see ourselves as part of an objective situation which yet must elude unique description is, I think, to be aware of a genuine mystery.

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Summary:

It is sometimes suggested that physical determinacy in the brain would prove our subjective conviction of freedom to be illusory. Even though it is granted that an observer's prediction could not be shared with the agent and remain valid, it is held that the agent would be merely ignorant of the true state of affairs.

The present paper contends (*a*) that the observer's view in this case cannot claim to represent uniquely the true state of affairs, (*b*) that despite linguistic appearances there is no predictive information in the observer's view of which the agent may properly be said to be ignorant.

The suggestion emerges that in this domain conventional ideas of arriving at the truth by comparing notes require to be radically revised.

University of London

IV.—TYPES AND MEANINGLESSNESS

By Arthur Pap

ARTHUR PAP (1921–1959)

Arthur Pap died in New Haven, Connecticut, on September 7, 1959, at the age of thirty-seven. After attending secondary schools in Zurich, his birthplace, he came to the United States, where for a short time he studied music at the Julliard School, and then philosophy at Columbia University. He received his Ph.D. at the age of twenty-three, and his dissertation (*The A Priori in Physical Theory*, 1946), together with his other books (*Elements of Analytic Philosophy*, 1949; *Analytische Erkenntnistheorie*, 1955; *Semantics and Necessary Truth*, 1958), became well known among analytic philosophers. He was a frequent contributor to learned journals, and just before his death he had completed a fifth book, on the philosophy of science. His lamentably short teaching career included appointments at the Universities of Columbia, Chicago, Oregon, Vienna, and at Yale University, where he taught for the past four years.

A. R. A.

It is characteristic of Russellian logic that a limited "range of significance", in Russell's phrase, is associated with every predicate. Such a range is a class of entities such that the predicate is true or false of all and only members of that class. The context in which Russell came upon the distinction between false and meaningless predications was the problem of solving, or avoiding, logical paradoxes. Thus, by ruling that the predicate "is not predicable of itself" is neither true nor false of that very property it seems to designate—indeed, is not significantly predicable of anything (hence does not really designate a property)—he prevented the deduction of the contradiction that there is a property, *viz.* impredicability, which both is and is not predicable of itself. But the distinction between the false and the meaningless is already recognized by common sense apart from any preoccupation with logical theory. "The theory of relativity is blue", "the number 5 weighs more than the number 6" "his mind eats fish": these and millions more predications would unhesitatingly be dismissed as meaningless, not false, in spite of their *syntactic* correctness, by plain people. In ordinary parlance, a rejection of a statement of the form "x is blue" as false is equivalent to the assertion that x has some

colour other than blue; similarly, were one to reject "x weighs more than y" as false, one would implicitly assert that either x and y have the same weight or y weighs more than x; and so on. In Russell's terminology: common sense recognizes the class of coloured entities as the range of significance of "blue", the class of entities that have weight as the range of significance of "heavy", and so on. For brevity, the range of significance associated with a given predicate will henceforth be called the "type" associated with it.

It is, however, easier to illustrate this concept of type than to explicate it. Russell's own definition, as I shall endeavour to show, entails a paradoxical consequence, to overcome which we must lift the restrictions imposed by the theory of types on significant discourse in an essential respect. I shall propose an alternative definition of "type" which seems to me to come much closer to what philosophers mean by this word and its synonym "category". In conclusion I shall give a fair hearing to the complaint that even this alternative definition is, from the point of view of a general classificatory "ontology", too restrictive. The verdict will be that the complaint is just as against the logical positivists' identification of the false with what is either empirically false or formally contradictory, but becomes groundless once type confusions are recognized as a new species of falsehoods that do not fit into the pigeon holes provided by early logical positivism.

So far only the expression "the type of the arguments of predicate P" has been explained, which is different from giving a general definition of "type", *i.e.* a statement of a necessary and sufficient condition for the typehood of a class. The former explanation corresponds to the following (contextual) definition of "type" in *Principia Mathematica*, *63: the type of the members of α is the logical sum of α and its complement. But the authors also provide an *explicit* definition: a type is a class such that the assertion that something is a member of it is true provided it is significant. Another explicit definition of the Russellian concept of type which is formally similar to Russell's definition of number goes: a type is a class of all entities that are of the same type as a given entity. The concept of significance occurs in this definition as well, for x and y are of the same type, according to Russell, if "x" can be substituted for "y", and conversely, in any significant sentence without producing nonsense. We shall see that this criterion of sameness of type is needlessly counterintuitive. Indeed, according to the alternative definition of type proposed below, two entities which belong to a common type may also

belong to different types ; the expression " is of *the* same type as " then becomes as improper as the expression " belongs to *the* same class as ".

I propose to show that, when combined with statements to the effect that a given class is a type, *Principia's* explicit definition of typehood leads to unacceptable consequences. Let us see how one might prove by means of Russellian type theory that the sentence " Socrates is a prime number " (S) is meaningless. Assuming that the type of the arguments of the arithmetical predicate " is prime " is the class of all natural numbers, we can write down the premise :

(*a*) S is meaningful if and only if Socrates is a natural number.

In order to pass to the desired conclusion that S is meaningless we require the further premise.

(*b*) Socrates is not a natural number.

But we can hardly prove the meaningful conclusion that S is meaningless from a meaningless premise ; hence this very proof of the meaningfulness of S presupposes the meaningfulness of (*b*). Now, if the natural numbers constitute a Russellian type—a plausible assumption (since the predicate " is prime ", in its arithmetical sense, is normally restricted to natural numbers), which would be reflected in a formalized language by the occurrence of special variables ranging over the natural numbers, then " Socrates is a natural number " is true provided it is significant. And since it clearly is not a true sentence, it must be meaningless. But the negation of a meaningless sentence is surely itself meaningless : the relevant sense of " meaningless " here is " neither true nor false ", and by the principles of excluded middle and non-contradiction, if p is either true or false, then not-p is true or false ; by contraposition, if not-p is neither true nor false, then p is neither true nor false. Therefore *Principia's* explicit definition of type has led us to the conclusion that (*b*) is meaningless, though our proof of the meaninglessness of S presupposes the very opposite. Obviously, the difficulty here illustrated besets any demonstration of meaninglessness arising from type confusion that follows the same pattern.

Might we solve this contradiction by adopting the formal mode of speech ? Let us replace " Socrates is a natural number " in (*a*) by " 'Socrates' is the name of a natural number ", and similarly (*b*) by " 'Socrates' is not the name of a natural number ". We then derive the meaningful metalinguistic conclusion that S is meaningless from metalinguistic premises whose obvious meaningfulness is *prima facie* consistent with Russellian type theory. This reformulation of the proof of meaninglessness

corresponds to the view that it is expressions, not extralinguistic entities, that are to be segregated into types. But unfortunately the difficulty cannot be solved by taking this fashionable way out. How do we know that "Socrates" is not the name of a natural number? Surely this knowledge is derived from our knowledge that it is the name of a person and that no persons are natural numbers, or indeed numbers of any sort. Thus, once we try to produce the obvious justification for our semantical premise, we find ourselves driven to the ontological statement "no persons are natural numbers" which it is impossible to formulate in a Russellian type-theoretical language: "(x) (person (x) ⊃ ~ natural number (x))" is nonsense because "x" in the antecedent has a different range from "x" in the consequent.[1] In general, it is impossible to formulate statements *about* types within a type-theoretical language, nor can the difficulty be overcome by talking about types semantically. Thus, consider the theorem of Russellian type theory that distinct types do not overlap. In a language employing variables of quantification it reads: for any classes A and B, if A is a type and B is a type and A is distinct from B, then there is no x such that x is a member of both A and B. Clearly we here refer to all classes, and indeed all entities whatever, which is meaningless according to Russellian type theory. But a semantical formulation of the theorem is equally unfeasible. We might try to put it this way: if the *expressions* "P" and "Q" are different type predicates (like "individual" and "class of individuals"), then there is no meaningful, and *a fortiori* no true sentence of the form "Px . Qx". But actually this does not accurately express our meaning. Suppose there occurred a conjunction "Pa . Qa" where "P" and "Q" are different type predicates. It would be meaningless, according to Russellian type theory, only if "a" designated the same entity in both occurrences. And this semantical condition cannot be expressed without the use of an unrestricted variable ("entity"); hence it cannot be expressed in a logically perfect language of the Russellian kind. The old difficulty of how to formulate Russellian type theory without violating it in the very act of formulation has reappeared.[2]

It is important to be clear about the difference between a *semantical* and a *syntactical* formulation of type restrictions.

[1] *Cf.* R. Carnap, *Logical Syntax of Language*, sec. 77.

[2] That semantical, as distinct from syntactical formulations of type restrictions defeat their own purpose, was first called to my attention by my colleague F. B. Fitch. He is not, however, responsible for any of the specific arguments in the text.

In order to prevent the emergence of the Russell paradox within a formal, uninterpreted calculus one may simply stipulate that expressions of the form "$x \epsilon x$" (and certain other "unstratified" strings of symbols) are not to be admitted as formulae or sentences. But in order to restrict the expressions that are substitutable for a variable "x" in the context of a descriptive sentential function to such expressions as will yield meaningful sentences, one must obviously refer to the *meanings* of expressions. To suppose that one can refrain from talk about types of entities by substituting talk about types of expressions is to overlook that to speak of the type of an expression is to speak elliptically of the type of the designated entity. To say that "Socrates" is a personal name and therefore may not be combined with an arithmetical predicate, such as "is a prime number", is to say that "Socrates" is the name of a person and hence may not be substituted for a variable ranging over numbers. And how can one prove from the premises that "x" ranges over numbers and that Socrates is a person, that Socrates is not a value of "x" if one is not permitted to assert that no persons are numbers?

There is, indeed, a formal trick by which consistency between the Russellian conception of types and the described method of proving that a type confusion has been committed can be restored.[1] For premise (*a*) let us substitute: S is meaningful if and only if "Socrates is a natural number" is true. And for (*b*) let us substitute: "Socrates is a natural number" is not true. We can now pass to the desired conclusion that S is meaningless without having had to *use* the allegedly meaningless sentence "Socrates is not a natural number". If this sentence, as well as its negation, is neither true nor false, then it is not true; and since it is true to say that it is not true, this is *a fortiori* a meaningful statement in the meta-language. But though we thus formally avoid the admission of such sentences as "Socrates is not a natural number" as meaningful, such a device looks suspiciously like a *deus ex machina*. The fact remains that the natural answer to the question "how can you infer from the premises that 'x' in the context 'x weighs 10 lbs.' ranges over physical objects and that Beethoven's Fifth is a symphony, that Beethoven's Fifth is not a value of 'x' in that context?" is: because symphonies are not physical objects. We may, to be sure, derive the semantical conclusion "'Beethoven's Fifth is a physical object' is not true" from the semantical premises "the general noun 'symphony' does not denote physical objects"

[1] This possibility was called to my attention by Hilary Putnam.

and "the proper name 'Beethoven's Fifth' is the name of a symphony". But there are two reasons why such semantical formulations do not make the corresponding ontological statements dispensable.

First, the semantical statement "the general noun 'symphony' does not denote physical objects" is not synonymous with the ontological statement "symphonies are not physical objects". For it is a necessary fact that symphonies are not physical objects whereas it is a contingent fact of linguistic usage that the word "symphony" is not used to denote physical objects. Secondly, and more importantly, the semantical premise which is obviously needed to justify the semantical conclusion that "Beethoven's Fifth weighs 10 lbs." is meaningless, *viz.* "'Beethoven's Fifth' does not denote a physical object", cannot be formulated in the sort of *logically correct* language under criticism unless the semantical predicate "denotes" is *ad hoc* stipulated to be systematically ambiguous (in Russell's phrase). For suppose it were univocal. The semantical relation of denoting would then have as its converse domain a class containing as members entities of different types, like individuals, attributes of individuals, classes of classes of individuals, etc. Russell recognized that the theory of types entailed the systematic ambiguity of "meaning".[1] But unfortunately the theory of types even prevents us from stating the *reason* why two given tokens of the word-type "denotes" must be postulated to differ in meaning. Consider the sentences "'Socrates' denotes an individual" and "'5' denotes a number". Why must we assume that "denotes" is used in different senses in the two sentences? Because individuals and numbers are entities of different types, but this very statement obviously involves a completely unrestricted variable ("entities"), hence is nonsense according to the theory of types.

Abandonment of the very distinction between types and classes that are not types is an understandable reaction to such difficulties. Quine and White,[2] reacting against Russell and Carnap, recommend that we treat such impressive ontological predicates as "is an individual", "is an attribute", "is a number", as logically or semantically on a par with such less impressive predicates as "is an American", "is blue", "is

[1] See his "Logical Atomism" (1924), reprinted in B. Russell, *Logic and Knowledge* (New York, 1956, ed. R. C. Marsh), pp. 332-333.

[2] W. V. Quine, "On Carnap's views on ontology", *Philosophical Studies* (October 1951) (vol. ii, no. 5); M. G. White, *Toward Reunion in Philosophy* (Cambridge 1956), chap. 5.

divisible by 3", etc. If so, one's symbolic language—if he happens to be a symbolic logician—will contain variables ranging over all entities whatever, or at least there will be no philosophical objection to the use of such a language. And the law of the excluded middle, for predications, can assume the sweeping and compact form: for all entities x and for all properties P, either x is P or x is not P. "Socrates is a prime number", then, is simply false, the truth being that Socrates is not a prime number.

But is this course of rejecting the distinction between false and meaningless predications, except as a distinction relative to arbitrary linguistic conventions, really inevitable? Is it not an undeniable fact that people who have no philosophical axe to grind are inclined to reject some predications as meaningless, not false, on the ground that the subject does not belong to the type required by the predicate? Though the Russellian definition of typehood is, as shown, inadequate, it does not follow that the concept of type is obscure beyond hope. Still, I do not see how the demonstrated difficulty can be overcome except by straining one's semantical tolerance and allowing that all type predications —as I shall call predications of a type predicate—are meaningful whether or not they be true. According to Carnap "Socrates is an individual" is analytic, hence meaningful; but then it is incongruous to condemn the sentences "Socrates is an attribute", "Socrates is a proposition", "Socrates is a relation", which seem to be incompatible with the former, to insignificance. And if they are allowed to be meaningful, "Socrates is not a natural number" must likewise be admitted as meaningful, hence we can argue without paradox that "Socrates is a prime number" is meaningless *because* only natural numbers are either prime or non-prime and Socrates just is not a natural number.

That the meaninglessness of a sentence should result from nonlinguistic relations between the designata of its constituent terms—such as Socrates not having that property which everything that is either prime or non-prime has—is no more surprising than that the logical impossibility of what a sentence describes should result from such relations. Thus "that is a round square" describes a logical impossibility because the property conventionally connoted by "round" is incompatible with the property conventionally connoted by "square". And that one sentence may lack a truth-value *because* another sentence is false, is no novelty either. We have heard a good deal in recent discussions of "presuppositions" of the truth-or-falsehood of a statement. "The king of Switzerland is a pipe smoker" presupposes that

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there is a king of Switzerland, "Mr. Miller has stopped beating his wife" presupposes that there is a man named "Miller", that this man has a wife, and that there was a time when he did beat his wife. In these examples the presupposed propositions are contingent, to be verified empirically. The statement "*a* is red" may be said to presuppose in the same sense that *a* is a visible or spatially extended entity, *i.e.* it has a truth-value only if *a* is such an entity. But here the presupposition is not contingent. Whether or not it is fulfilled must be self-evident (in more emancipated terms, it must be evident to one who understands the language): it is self-evident that a dress, the specific colour of which must be discovered empirically, has some colour or other, that a number is not visible, that *the earth is round* is a proposition but not a number, etc.

The following alternative definition of type is herewith proposed: *a type is a class such that there are families of predicates which can be significantly*, i.e. *correctly or falsely, ascribed to all and only members of it.* A predicate family is a set of predicates such that one and only one member of it must be true of anything of which some member of the set is true or false. Simple sounds constitute a type relative to pitch predicates, smallest visible spots relative to colour predicates, propositions relative to truth-value predicates but also relative to predicates designating cognitive attitudes (like "is believed by A at time t", "is disbelieved by A at time t", etc.), integers relative to odd and even, arguments relative to valid and invalid, and so on. Type predicates are significantly applicable to everything: this is the main feature distinguishing the proposed definition of type from the Russellian. The difference will become apparent if we remember that according to Russell a type can be defined as the logical sum of any proper subclass of itself and the complement of that subclass, *e.g.* individuals as the logical sum of men and non-men—where the class of non-men is the class of entities such that "x is a man" is *false*, a class which excludes according to Russellian type theory numbers, propositions, attributes, etc. Similarly, Carnap wrote in his *Logical Syntax of Language* that a universal word (here called "type predicate") can be defined by selecting any predicate of the appropriate genus and forming an L-true disjunction; for example, "x is a natural number = (x is prime or x is non-prime)". But this mode of definition is illicit if type predicates are to have an unrestricted range of significant application. For, to consider again the example "Socrates is a natural number", it is a contradiction to hold that this sentence is meaningful, that "Socrates is a prime number" and "Socrates

is a non-prime number" are meaningless sentences, and also that "natural number" is definable as "prime or non-prime". No such contradiction arises in the type-theoretical languages favoured by Russell and Carnap because there "Socrates is a natural number" is just as meaningless as "Socrates is a prime number". Indeed, Carnap offered a criterion of universal words, in his *Logical Syntax of Language*, which is closely similar to the explicit definition of typehood in *Principia Mathematica* and which entails that a type predication which is not true is meaningless: if "P" is a type predicate ("universal word"), then all admissible substitution instances of "Px" are analytic sentences; therefore there are no *false* (not even "L-false") substitution instances of "Px" if "P" is a type predicate.

We must now address ourselves to the question whether in readmitting, contrary to Russell's theory of types, unrestricted variables associated with type predicates we may not be on the road to the Russell paradox, or a variant thereof, again. I should certainly retract my proposed alternative if it were shown to lead to contradiction, even though statements of completely unrestricted generality, like statements *about* types, are intuitively meaningful. Consistency is a requirement, I should concede to formal logicians, that has priority over the requirement of intuitive plausibility. Now, if every sentence of the form "$x \epsilon t$", where "t" represents a type predicate, is meaningful, then sentences of the form "$t \epsilon t$" are meaningful. Is it then meaningful to say that the class of all self-excluding classes is a self-excluding class? The answer depends on whether that troublesome class is a type in the new sense. But it seems to me obvious that whatever predicate applies significantly to self-excluding classes also applies significantly to self-including classes; if so, the class in question is not a type in the new sense. Similarly, there is no reason to fear trouble from the class of all self-excluding types, for it is not a type. For example, the predicate "is a type" must apply significantly to non-types as well as to types, otherwise it could not be *false* to say of a non-type that it is a type. Certainly, precautions against logical paradoxes must be taken. But it is by now well known that there are effective precautions other than the simple theory of types, in particular, precautions that do not involve restriction of the ranges of variables to entities of a definite type. One such alternative is the theory of J. von Neumann and Quine according to which classes whose definitions entail contradictions are, by stipulation, not members of any class.[1]

[1] See W. V. Quine, *Mathematical Logic*, revised edn. (Cambridge 1951).

Although types in the new sense are far more numerous than the "categories" of various ontologists, I do not think that by an arbitrary choice of predicate family *every* class can be made out to be a type. Professor Max Black called my attention to the fact that by admitting predicates of the form "has relation R to every *other* member of K" (*e.g.* "is admired by every other logician") one can obtain curious types on my definition. Thus, "is admired by every other logician" is true or false of logicians only, since any significant application of this predicate presupposes that the subject is a logician; hence the class of all logicians becomes a type! But let us analyse what we *assert* about x in saying "x is admired by every other logician": for every y, if y is a logician and y is different from x, then y admires x (that x is himself a logician is presupposed rather than asserted). It then appears that the range of values of x extends far beyond the class of logicians.

Obviously types in the new sense need not be hierarchically ordered. Thus sounds, visible things, propositions, arguments, natural numbers, physical motions, which classes can plausibly be regarded as types, cannot be arranged in any natural order. Nor need distinct types be mutually exclusive. Physical objects constitute a type because weight predicates and predicates of location in physical space are restricted to physical objects. But there are also predicate families which are restricted to animals: if it is meaningless to ascribe a weight or a location in physical space to a thought, or to a number, it is equally meaningless to say of a stone that it drinks milk or that it expects food, etc. Hence one type may be a proper subclass of another. Russell's conception of types as constituting a hierarchy of non-overlapping classes is hard to reconcile anyway with his own definition of "same type" (translated into the formal mode of speech above, p. 43): "A and B are of the same logical type if, and only if, given any fact of which A is a constituent, there is a corresponding fact which has B as a constituent, which either results by substituting B for A, or is the negation of what so results."[1] Surely individuals in the sense of *Principia Mathematica*, *viz.* entities of lowest type (formally characterized as entities which are not values of "y" in "$x \epsilon y$"), do not constitute a type in that sense; that is, not all individuals are of the same type according to this criterion. For example, let A be a tennis ball and B a mountain. It may be a fact that John caught the tennis ball, but could it possibly be a fact that John caught, or failed

[1] "Logical Atomism", *loc. cit.* p. 332.

to catch (in the same sense of the verb) a mountain? Again, colours and shapes are commonly lumped together by logicians as entities of the same type, *viz.* attributes of the first level (attributes of individuals), yet, while a colour can be said to be bright one would be at a loss to understand a sentence like "triangularity is bright".

The new conception of types is, furthermore, free of a highly unplausible feature of Russellian type theory: that where *a* and *b* are entities of different type, there is no predicate whatever which is significantly applicable to both *a* and *b*—not even "is thought of", or "belongs to a type to which some other entity does not belong". But above all, the new conception may be recommended as being in better accord with Carnap's own "principle of tolerance" with respect to language construction than his treatment of universal words as "quasi-syntactic" (or, for that matter, "quasi-semantic") predicates that should be banished from logically correct object-languages. To be sure, an object-language with suitably restricted variables can *ipso facto* dispense with the corresponding type predicates. If, for example, it is understood that "anything" refers only to bodies, then "anything which is less dense than water floats on water" will convey exactly the same information as "any body which is less dense than water floats on water". But there is no cogent philosophical objection to languages in which type predicates are employed in the company of unrestricted variables. Indeed, the use of type predicates in an object-language calls for the totality of entities as the range of the variables, precisely because type predicates, according to the new conception, are significantly applicable to every entity.

Yet, the proposed liberalization of type restrictions may be argued to be still not liberal enough. For, consider a categorizing statement like "all roses are things". Let it be transcribed into the hypothetical form favoured by symbolic logicians: "for any x, if x is a rose, then x is a thing". Such a statement is analytic in the same sense as "all dogs are animals", "all triangles are plane figures" are analytic statements: it unfolds a part, though very little, of the meaning of the subject-term, by specifying the categorial *genus* of the subject. Offhand it may seem that the variables in these statements could be restricted to the appropriate types. But if they were thus transcribed by means of restricted variables, they would collapse into formal tautologies. For example, "for any *thing* x, if x is a rose, then x is a thing" is equivalent to "for any entity x, if x is a thing, then: if x is a rose, then x is a thing", and here "rose" has

obviously a vacuous occurrence. The statement's intention of explicating a little of the meaning of the subject-term "rose" is lost in this transcription by means of a typically restricted variable. Accordingly we ought to write "for any x, if x is a rose, then x is a thing" where "x" is completely unrestricted. On the other hand, there is no harm in equipping the same predicate, "is a rose", with a restricted argument variable in statements that do not contain type predicates, like "there is a thing, x, such that x is a rose and x is red". To add an illustration, consider the relational predicates "precedes" and "happens at the same time as". Intuitively, they can be significantly applied to pairs of events only, and variables restricted to events are hence called for in a context like "for any x and y, if x precedes y, then y does not precede x". But in order to define the class of events as the field of the relation of temporal succession, we need an unrestricted variable precisely because we are introducing a type predicate: x is an event = df (Ey) (x precedes y or y precedes x).

Now, look at the latter definition a little more closely. Suppose we wanted to use it to determine whether a given entity is an event. For example, "Africa is an event" is synonymous, according to this definition, with "there is something which is earlier or later than Africa". In order to move to the conclusion that Africa is not an event, we have to take this odd statement as false, not meaningless. But this contradicts our inclination to accept statements of the form "x is earlier than y" as meaningful only if x and y are events. Similarly, from "for any entity x, if x is a rose, then x is a thing" we derive by substitution the odd statement "if the philosophy of logical atomism is a rose, then it is a thing". The following moral is to be drawn from this puzzle: though the word "meaningless" in the sense of "neither true nor false" has a perfectly good use in ordinary language, philosophical analysis forces upon us the conclusion that the line between the false and the meaningless is not only fuzzier than it is according to the Russell-Carnap theory, but even fuzzier than it appears in the liberalized conception of types. The meaninglessness illustrated by "the philosophy of logical atomism is a rose" and "the theory of relativity is a good swimmer" could, after all, be called "*a priori* falsehood" by those who do not wish to call a sentence meaningless unless at least one constituent term is meaningless. Such *a priori* falsehood is very different from formal contradiction, but the question whether "false" or "meaningless" is the right word to use is just as verbal as the question whether formally contradictory

statements are false or meaningless. Meaninglessness consisting in type confusion is, so to speak, the negative side of non-formal necessary truth. Thus, if "whatever is red, is extended" is necessary without being formally analytic, then "some entity which is not extended is red" should be *a priori* false without being formally contradictory—a "synthetic *a priori*" falsehood. Indeed, the latter statement is formally entailed by the conjunction of "no numbers are extended" with the supposedly meaningless statement "the number Five is red"; hence we might as well call it meaningless in the same sense. And if we do call it so, we should also call its negation, "whatever is red, is extended", meaningless. This indicates that in preferring the trichotomous classification of sentences constructed out of meaningful terms as true, false or meaningless one manifests the tendency—noticeable in Wittgenstein's *Tractatus*—to use "true" and "false" in the sense of *contingent* truth and falsehood. A true statement, on this view, is one which corresponds to a fact which might not have been, and a false statement is one which describes a conceivable fact that happens not to exist.

Those philosophers, however, who find no fault with the admission of *a priori* insights that are not reducible to recognition of logical (formal) truth or falsehood—I count myself among them—may prefer to call the statement "the number Five is red", not meaningless, but a synthetic *a priori* falsehood, *i.e.* a statement the negation of which follows from the synthetic (non-formal) necessary truths "Red is a colour" and "no number has a colour". This would amount to treating the sort of meaninglessness which was to be avoided by type restrictions as a special sort of falsehood, different from both formal contradiction and contingent (empirical) falsehood. Instead of the distinction between false and meaningless predications one would then have to observe the distinction between two senses of "not" or "false" as prefixed to a predication. Ordinarily "not-(S is P)" is used synonymously with "S is non-P" where "non-P" is equivalent to the disjunction of all the other members of the predicate family to which P belongs. As noted already, a denial of "x is blue" is commonly regarded as equivalent to the affirmation that x has some colour other than blue. Similarly, to deny that x is kind is to affirm that x is unkind, to deny that x is now in New York is to affirm that x is now somewhere else, and so on. Now, if we wish to admit "Five is red" as a false statement, in other words, its negation as a true statement, we may do so provided we introduce a new kind of negation, a negation which does not satisfy the rule that to deny "S is P"

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is equivalent to affirming "S is non-P" where "non-P" signifies the disjunction of all the other predicates belonging to the same family as P. The new kind of negation might be called *unlimited*, in contrast to *limited* negation, *i.e.* negation within a limited genus. Correspondingly, we might distinguish limited from unlimited falsehood of a predication. In the proposed definition of type (p. 48) one would then have to substitute for "false", "limitedly false"; and sentences hitherto called "meaningless" in the sense of type confusion would be dignified with unlimited falsehood.

The distinction between limited and unlimited negation enables me to clarify an important reason behind my proposal to accept all type predications, regardless of the type of the subject, as true or false while withholding "false" from predications which fail to be true because a presupposed type predication is false. It is simply that in the case of non-categorial predications a negation is ordinarily construed as a limited negation and hence stimulates a search for the true predication though there is no true positive predication which is the ground of the denial. If I deny that my thoughts are in my brain, I am likely to be challenged to specify their true location, though my intent was to deny that thoughts have any spatial location at all. If I deny that the molecules of which the air is composed are themselves hot, I might be taken to mean that they are cold, or lukewarm, or cool. In the case of a type predication, on the other hand, "not-(S is P)" *is* equivalent to "S is non-P", and for this reason a denial of "Socrates is a number" is intuitively more plausible than a denial of, say, "Socrates is divisible by an even square number". Having denied a type predicate P to an entity, I can go on and ascribe a type predicate incompatible with P to it: Socrates is not a number but a person, modesty is not something that has shape but a form of behaviour, molecules are not perceptual objects but postulated scientific objects, thoughts are not physical events in physical space but mental events. The locution "x is not the sort of thing to which predicate P can be ascribed", which is frequently used by ordinary-language-analysts who caution against category mistakes, expresses nothing else than the feeling that "x is P" is meaningless, or unlimitedly false, because a presupposed type predication is false.

Yale University

V.—EXTENSIONALITY

By Ruth Barcan Marcus

The continued development of intensional logics, and concern with problems of their interpretation has had a rather curious effect. It has reinforced the notion, unjustifiable in my opinion, that extensionality is an unambiguous concept. This presumed clarity is usually singled out as the virtue of extensional systems, to say nothing of their metaphysical advantages. The assertion that in mathematics and empirical science one does not *need* to traffic in non-extensional notions which are fuzzy and troublesome, has become a virtual platitude. Yet a cursory examination of the literature does not reveal any well-defined theory of extensionality, although it is possible to find a core of agreement. Indeed, there are differences as to (*a*) what are the principles of extensionality, (*b*) which objects are or ought to be extensional, and (*c*) which formal systems are extensional.

My purpose in this paper is to arrive at a characterization of extensionality in terms of these differences which may be helpful in connection with some familiar problems of interpreting intensional systems.

Principles of extensionality. Consider first some unspecified system of material implication L with theory of types. On the propositional level, extensionality takes the form of a substitution principle :

> If p is equivalent$_1$ to q then A is equivalent$_2$ to B,
>
> where B is the result of replacing one or more occurrences of p in A by q. (1)

As stated, (1) is of course ambiguous. The ambiguity concerns the meaning of " equivalence$_1$ " and " equivalence$_2$ ". A minimal requirement of an equivalence relation is that it be reflexive, transitive and symmetrical. These conditions are met by a variety of relations ranging from identity to having the same weight, and further interpretation is required. Our concern is with logically definable relations of equivalence.

Using the abbreviations " eq$_1$ " and " eq$_2$ ", let us first consider principles in which eq$_1$ and eq$_2$ have the same meaning. If they are taken as identity, (1) becomes

> If pIq then AIB (where " I " names the identity relation) (1.1)

and is merely explicative of the notion of identity. Suppose what is intended is

> If "$p \equiv q$" is a tautology then "$A \equiv B$" is a tautology. (1.2)

In what sense is (1.2) an extensionality principle? Only in that it eliminates as possible predicates of propositions, certain intensional predicates such as "believed by John". Not all intensional predicates are precluded by (1.2). In particular, modal predicates such as "logically necessary" would not falsify (1.2). Ordinarily, variables which range over predicates of propositions are dispensable, and consequently (1.1) is often provable as a strong form of the substitution theorem.

Most commonly, eq_1 and eq_2 are interpreted as material equivalence without the modifying condition of (1.2):

$$\text{If } p \equiv q \text{ then } A \equiv B. \tag{1.3}$$

Here (1) is taken to mean that if p and q have the same truth value, whether contingently or necessarily, than A and B have the same truth value. As contrasted with (1.2), (1.3) is a strongly extensional principle for it disallows all intensional predicates of propositions. Here again, where variables which range over propositional predicates are not introduced, (1.3) is provable as the substitution theorem.

Consider, again on the level of propositions, principles in which eq_1 and eq_2 are not the same. If eq_1 is taken as identity and eq_2 material equivalence, then (1) becomes

$$\text{If } pIq \text{ then } A \equiv B. \tag{1.4}$$

(1.4) like (1.1) is explicative of the identity relation. [The converse of (1.4) is of course another matter involving as it does the assumption of Leibniz's law, in addition to being strongly extensional.]

If we take eq_1 as material equivalence and eq_2 as identity, we have

$$\text{If } p \equiv q \text{ then } AIB \tag{1.5}$$

which in the first instance, where A is p, becomes

$$\text{If } p \equiv q \text{ then } pIq. \tag{1.51}$$

In contrast to (1.4), (1.5) is very strongly extensional since it not only eliminates intensional predicates of propositions but assimilates propositions to truth values.

What I am trying to make apparent by this necessarily crude and informal analysis, is that even on the level of propositions, we cannot talk of *the* thesis of extensionality but only of stronger

and weaker extensionality principles. I will call a principle *extensional* if it either (*a*) *directly or indirectly imposes restrictions on the possible values of the functional variables such that some intensional functions are prohibited or* (*b*) *it has the consequence of equating identity with a weaker form of equivalence.* Obviously (*a*) and (*b*) are interdependent. On the basis of this characterization, (1.2), (1.3), and (1.5) are all principles of extensionality, in order of increasing strength. It should now be clear why there is often disagreement as to whether a given formal system is or is not extensional. There is, for example, a literature of tiresome arguments as to whether the formal system of *Principia* is extensional. It is all a matter of deciding how extensional a system must be to be properly so-called. There are by contrast, logicians such as Alonzo Church who does talk in terms of degrees of extensionality. A more reasonable approach would be to assert, in connection with *Principia*, that the formal system as interpreted in the first edition is less extensional than the interpretation proposed by the second edition, since the latter assumes an analogue of (1.5) which is stronger than (1.2).

Consider next another set of principles which are more frequently associated with the theory of extensionality. Principles which relate the equivalence of classes (or attributes) to the equivalence of their defining functions.

$$\text{If } (x)\,(F(x)\ \text{eq}_1\ G(x)) \text{ then } F\ \text{eq}_2\ G. \qquad (2)$$

In addition to the interpretation of eq_1 and eq_2, one must specify whether F and G are predicate variables, class variables, or non-committal functional variables. I cannot give an exhaustive account of the many possible variations of (2). In a weakly extensional system, eq_1 might be taken as tautological equivalence, eq_2 as identity, F and G functional variables, as follows:

$$\text{If } (x)\,(F(x) \equiv G(x)) \text{ is tautological, then } FIG. \qquad (2.1)$$

By the criterion of extensionality stated above, (2.1) is weakly extensional since it precludes some intensional contexts. On the other hand (2.1) permits us to state an identity between the terms "9" and "3^2" but not "9" and (on the assumption that it can be construed as an expression of proper type level) "the number of planets". A stronger alternative (referred to most often as *the* extensionality principle) asserts identity of functions as a consequence of formal equivalence.

$$\text{If } (x)(F(x) \equiv G(x)) \text{ then } FIG. \qquad (2.2)$$

In languages which distinguish classes from attributes, the distinction is sometimes maintained by postulating (2.2) for

classes and perhaps (2.1) for attributes. This has the effect of eliminating intensional contexts involving class names but allowing such contexts for attribute names. Such is the interpreted procedure of *Principia*. The concept of identity in *Principia* is systematically ambiguous not only as prescribed by the theory of types, but on the same type level. In the second order predicate calculus, "identity" means something different for classes than for attributes, and has still another import for individuals. My preference is for the alternative procedure of giving uniform meaning to "identity" and to talk of attributes and classes as being equal, but not identical. Functional *equality* would be defined as

$$(F = G) \rightarrow_{df} (x)\,(F(x) \equiv G(x)) \qquad (3)$$

where $F = G$ is *not* equated to FIG. On the basis of known substitution theorems, the substitution of F for G in strongly extensional contexts is still permissible and in such contexts $F = G$ is *like* FIG. This permits us to say that the class of mermaids and the class of Greek gods are equal but not identical and that in strongly extensional contexts (arithmetic ones for example) we are concerned only with their equality, so that the name of one may be substituted for the other.

It seems to me that much of the discussion these past few years concerning apparent breakdowns of substitutivity principles in intensional contexts and its presumably devastating results for logic and mathematics are largely terminological. I am not (as Quine [1] insists in his review of two of my papers on quantified modal logic) proposing that there be more than one kind of identity, but only that the distinctions between stronger and weaker equivalences be made explicit before, for one avowed purpose or another, they are obliterated.

The usual reason given for reducing identity to equality [(3) (2.2)] is that it provides a simpler base for mathematics, mathematics being concerned with aggregates discussed in truth functional contexts, not with predicates in intensional contexts. Under such restrictive conditions, the substitution theorem can generally be proved for equal (formally equivalent) classes, with the result that equality functions *as* identity.

Establishing the foundations of mathematics is not the only purpose of logic, particularly if the assumptions deemed convenient for mathematics do violence to both ordinary and philosophical usage. I am not disturbed by the possibility of equal, non-identical classes or attributes, *e.g.* man and featherless biped.

[1] W. V. Quine, *Journal of Symbolic Logic*, xii (1947), 95-96.

To me it seems reasonable that there are many empty classes of the same type, *e.g.* mermaids and Greek gods, equal but not identical. And why should the non-identity of the numbers n and n + 1 depend on the enumeration of different things in the world? To subsume mathematics under logic is not to equate them. A much broader base is indicated in the direction of intensional systems such as the modal logics. I will try to show that the apparent difficulties of interpreting such systems are not genuine, but analogous to a rejection of a non-Euclidean geometry because it allows parallel lines to meet.

To complete this analysis, I must consider another set of modifications of identity involving Leibniz's law.

The identity of indiscernibles. Sometimes identity is introduced as

$$\text{If } (\phi)\ (\phi(x) \text{ eq } \phi(y)) \text{ then } x I y. \tag{4}$$

The usual interpretation of eq is as material equivalence:

$$\text{If } (\phi)\ (\phi(x) \equiv \phi(y)) \text{ then } x I y. \tag{4.1}$$

Another possibility is in terms of tautological equivalence.

$$\text{If } (\phi)\ (\phi(x) \equiv \phi\ (x)) \text{ is a tautology, then } x I y. \tag{4.2}$$

The converse of (4) is of course explicative of identity. The status of (4) itself is not quite so clear. It has also been accepted as a truism and merely explicative. However, I do not regard Ramsey's [1] reservations about (4) as entirely spurious. He objected to taking (4.1) as definitive of identity on the ground that it is logically possible for two things to have all their properties in common and still be two. Such a possibility is excluded by (4.1). To argue that if they are two then they are distinguishable as having two different names will not do for Ramsey, since they may be unknown, unnamed, and still two.

According to our characterization of extensionality, instances of (4) may therefore be interpreted as extensionality principles in that they equate identity with the slightly weaker relation of indiscernibility which requires that to be distinct means to be *discernibly* distinct.[2]

[1] F. P. Ramsey, *The Foundations of Mathematics* (London and New York, 1931), pp. 30-32.

[2] I am aware that interpreting (4) in this way is somewhat paradoxical since (4) has the effect of establishing a logical priority of the concept of "property" over that of "class" whereas in an extensional system the emphasis is held to be on the class concept. If such usage is intolerable the characterization of extensionality can be appropriately modified.

Interpreting intensional systems. Quine[1] states: "When modal logic is extended (as by Miss Barcan) to include quantification theory, . . . serious obstacles to interpretation are encountered." These difficulties revolve about the substitution of equivalences in contexts involving "knows that", "is aware that", and in particular "is necessary that", and "is possible that". Quine describes such contexts as being referentially opaque. It is the point of this paper to show that the opacity lies with Quine's use of such terms as "identity", "true identity", "equality". The above analysis leads to the dissolution of at least some of the problems of interpretation associated with intensional contexts.

Among the equivalence relations which can be introduced into L are identity, indiscernibility, tautological equivalence, material equivalence. On the functional level, these are listed in order of decreasing strength, for there is some model, some permissible range of values, which prevents our equating them except by explicit postulate. It should be noted that for variables of lowest type, there are only identity and indiscernibility. Indeed a recent paper of Bergmann[2] on *Individuals* may be understood as an attempt to explicate the notion of individuals as those entities for which only the strongest equivalence relation holds.

Consider now modal functional calculi such as my[3] extension of the Lewis systems. In such languages (2.1) can be stated directly as

$$\text{If } N((x)\,(F(x) \equiv G(x))) \text{ then } FIG \text{ (where N is interpreted as logical necessity)} \tag{5}$$

(4.2) becomes

$$\text{If } N((\phi)\,(\phi(x) \equiv \phi(y))) \text{ then } xIy \tag{6}$$

and (1.1) is

$$\text{If } N(P \equiv Q) \text{ then } PIQ. \tag{7}$$

Within these extended systems, I have been able to prove

[1] W. V. Quine, "The Problem of Interpreting Modal Logic", *Journal of Symbolic Logic*, xii (1947), 43-48.

[2] G. Bergmann, "Individuals", *Philosophical Studies*, ix (1958), 78-85. [My interpretation of this paper rests on the assumption that the statement of (Ext) involves a typographical error.]

[3] R. C. Barcan (Marcus): "A Functional Calculus of First Order Based on Strict Implication", *Journal of Symbolic Logic*, xi (1946), 1-16; "The Deduction Theorem in a Functional Calculus of First Order Based on Strict Implication", *ibid.* 115-118; "The Identity of Individuals in a Strict Functional Calculus of Second Order", *ibid.* xii. 12-15.

theorems which relate different kinds of equivalence. It is possible to show

> Given $P \equiv Q$, P is not everywhere interchangeable with Q, but only in restricted non-modal contexts. Given $N(P \equiv Q)$, then the substitution theorem is unrestricted. (8)

This theorem has the effect of prohibiting the substitution of "Socrates is a featherless biped" for "Socrates is a man" in "It is necessary that if Socrates is a man then Socrates is a man". That the substitution theorem for strict equivalence differs from the theorem for material equivalence, is not paradoxical, but a more adequate formalization of a known distinction.

It is when Quine [1] refers to

> The number of planets equals nine (9)

as a "true identity", without hint of ambiguity that we become aware that his fundamental criticism is directed not toward presumed paradoxes but toward the intensional point of view. As indicated above (9) is *not* unambiguous except in a strongly extensional language.

Let us assume for the moment that "9" and "the number of planets" are expressions of the same type level and can meaningfully be equated. Quite apart from interpretation in terms of the theory of descriptions, within the modal language the problem revolves about substituting "the number of planets" for "9" in

$$N(9 > 7). \quad (10)$$

But such a substitution is prohibited by (8), for (9) does not assert tautological equivalence, and the substitution would have to be made within the scope of a modal operator. The paradox evaporates. By the same token, since

$$N(9 = (5 + 4)), \text{ "5 + 4" can replace "9" in (10).} \quad (11)$$

The problem of the morning star and the evening star is resolved in an analogous way.[2] For, like (9),

> The evening star equals the morning star (12)

is not unambiguous. If (12) involves proper names of individuals then "the evening star" may replace "the morning star" without paradox in

> It is necessary that the evening star is the evening star (13)

[1] W. V. Quine, *From a Logical Point of View* (Cambridge, Mass., 1953), p. 144.

[2] The paragraph which follows restates a point made by F. B. Fitch in "The Problem of the Morning Star and the Evening Star", *Philosophy of Science*, xvi (1949), 137-140.

for the only equivalence relation between individuals are identity and indiscernibility. Indeed, although it appears as if (4.1) and (4.2) express two kinds of indiscernibility, they can be *proved* strictly equivalent within a modal system. Quine's [1] failure to note the latter in his review of my paper had the unfortunate result of perpetuating a non-existent paradox.

If, on the other hand, (12) is about classes or properties, then it states a non-tautological equality, not an identity, and consequently, the conditions of the substitution theorem (8) prevent the substitution of "the morning star" for one of the occurrences of "the evening star" in (13). At the risk of too much repetition, we are not asserting that the substitution *ought* not to be made on the basis of some pre-formal analysis, but that they are prohibited by the theorems provable [2] in such extended systems.

I have tried in this brief paper, to characterize the theory of extensionality, and to show that logical systems are more or less extensional. Their extensionality depends on the kinds of contexts and predicates which are prohibited, and the degree to which the relation of identity is equated to weaker forms of equivalence. I also tried to show that a more broadly based logic in the direction of modalities need not do violence to the foundations of mathematics, and the supposed paradoxes involved in interpreting such intensional systems are not genuine.

Roosevelt University

[1] See n. 1, p. 60. Quine's failure to notice that (4.1) and (4.2) are materially equivalent in $S2^2$ and strictly equivalent in $S4^2$ and $S5^2$ leads him to conclude that modal logic must deal with individual concepts rather than individuals. In a recent letter, Quine forwarded copies of a note to the editor of *The Journal of Symbolic Logic*, and his publisher correcting the error.

[2] The substitution principle (8) is a rough restatement of substitution theorems for some of the extended modal calculi. The theorems are proved at the end of the first paper listed in n. 3, p. 60.

VI.—DISCUSSIONS

PHILOSOPHERS AND PRESUPPOSITIONS

A TERM which recurs ever more frequently in the writings and conversations of philosophers is the term "presupposition". I find the term and its uses far from clear. I should like to suggest briefly some of the ambiguities and difficulties surrounding the term and its uses.

As I understand these discussions in which the term "presupposition" occurs, it most frequently refers to some belief, question, or statement. That is to say, a presupposition *is* a belief, question, or statement. (To the extent that the term is used to denote something radically different from any of these, my discussion will probably be largely irrelevant.) This being the case, I wish to consider who or what presupposes or has presuppositions. I shall suggest three alternatives which have been or might be proposed.

(1) One might say that any systematic and organized body of knowledge has presuppositions. A variant of this, and perhaps a preferable way of expressing it, would be to say that the persons possessing or developing such knowledge have certain presuppositions.

A distinction might have to be made between those who take this in an historical sense and those who believe it is in the nature of a body of knowledge, such as a specific science, that it has certain presuppositions. The latter may be what is sometimes meant by those who speak of "the philosophy of" this or that. As for the former, *i.e.* those who take this interpretation of presuppositions in an historical sense, their position has been set forth by Collingwood.[1] He defines "metaphysics" as an historical science which seeks to discover the presuppositions which were "had" or "made" by scientists in various periods.

I am not sure whether it makes sense to say, or whether it is true, that it is in the nature of a body of knowledge that it has certain presuppositions. Many philosophers have spoken and written as if this were true and as if their unique and primary task as philosophers were to call attention to these presuppositions—a task which the scientists themselves are supposedly too conceited or narrow-minded or busy to do. Since it is by no means clear what such presuppositions would be, whether methodological principles or ontological claims or some other kind of thing, I do not know what more can be said about this approach.

In my opinion the historical view proposed by Collingwood is more intelligible and plausible. I am not here concerned with such questions as whether Collingwood's use of the term "metaphysics" is a proper one and whether he is correct in saying that such philosophers as Aristotle and Kant, whatever else they may have thought

[1] R. G. Collingwood, *An Essay on Metaphysics* (Oxford University Press, 1940).

they were doing, were primarily engaged in doing metaphysics, as defined by Collingwood. I am more interested in whether it is plausible to say that by investigation we can discover that in certain periods the scientists working in a given field *did* assume or suppose or believe certain things to be true, often without their realizing it, and that these things led them to ask certain questions and make certain statements and believe certain things to be true. And of course these "presuppositions" also prevented them from asking certain other questions which might otherwise have been asked.

In a general way at least, I have no quarrel with the above view. We have ample evidence that competent historians and other investigators have been able to discover, and to state with considerable confidence, what things were accepted in certain periods as true, and that these largely determined the kinds of questions that were asked and the kinds of answers that were given.

(2) A second view might be that if we examine human beliefs, statements, and questions, we find all of them somehow "rest upon" or "derive from" a core of fundamental beliefs or assumptions, which are common and universal and which we might call "presuppositions".

This second view is somewhat more awkward to deal with than the first. For one thing, it is not clear who, if anyone, has held just such a view. One is tempted to say that this is the Kantian view; or at least it might be what some neo-Kantians have intended. To some extent at least, it *is* plausible to say that what Kant was trying to do was to show that there are certain beliefs which are universal and fundamental, such as the belief that every event must have a cause. Some philosophers have maintained that the "transcendental deduction" should be interpreted along these lines. Others would argue that Kant was not speaking of universal *beliefs* but of characteristics of a man as a perceiving and believing and knowing subject.

This much can be said here. If the latter is what is meant, these "characteristics" would not fall within the meaning of "presupposition" as I am using the term in this paper (*i.e.* to refer to a belief, statement, or question). One might even go on to suggest that to the extent that we are seeking for universal "characteristics", it is not clear why this should be within the province of the philosopher, rather than the psychologist, the anthropologist, or the biologist.

On the other hand, if there *are* certain assumptions which are universally made or beliefs universally held, and if these "account for" various beliefs, statements, or questions, and if they are what are meant by "presuppositions", it would doubtless make sense to say that they should be discovered and articulated if possible. And if the philosopher is in some way capable of performing this task, it would be important that he recognize and execute it.

(3) There is a third possibility which someone might propose. It is that if we consider a belief which a certain person has or a statement which he makes or a question which he asks, we may often

discover that the belief, question, or statement has presuppositions. Or we might say that such a person presupposes certain things.

It might be suggested that this is what Socrates was doing, among other things. That is, perhaps Socrates was trying primarily to discover, by skilful questioning and comment, what led people to say and believe certain things; or perhaps more importantly, he was trying to lead people to see for themselves why they said and believed certain things. Once again, I am not here mainly concerned with the historical question of whether this was in fact what Socrates was doing. I myself should say that whether he was or not, the activity in question has considerable merit. To the extent that a person can be led to understand why he says or believes or asks what he does, he will be a wiser person. And one could do worse than to describe the work of a practising philosopher or teacher of philosophy as consisting largely in trying to bring about this kind of self-knowledge.

It seems to me that a variant of this view is involved in Strawson's discussion of presuppositions.[1] He states that the traditional four-fold classification of categorical propositions and their relations in the Square of Opposition can be saved from breakdown if we recognize that the entire system *presupposes* the existence of members of the subject-class. He is careful to insist that the relation is not that of logical entailment. Nevertheless he insists that some sort of logical relation is involved, which he designates variously as "precondition", "necessary condition", or "presupposition". Strawson's proposal is a suggestive one and merits further attention and elaboration.

We have, then, at least three alternative views, each of which maintains that we can begin with certain beliefs, statements, or questions, and discover others which have to them a relation of some kind of priority—logical, if not psychological or temporal. Other alternatives than these three might be suggested, and the ones that have been mentioned could be elaborated. The thing I wish to emphasize here is that, at least initially and generally, each of these has a fair degree of plausibility and offers some promise of being carried out with success. Moreover, each of them would be important.

I come now to a view which bears some resemblance to those mentioned above, but which is more pretentious than any of them—and less plausible.

Again I shall refer to Collingwood for a representative statement of the view in question.[2] In a chapter called, "On Presupposing", he states that every statement is an answer to a question and every question arises from some logically prior statement. He then goes on to explain that in so far as something is an answer to a question it

[1] P. F. Strawson, *Introduction to Logical Theory* (London: Methuen & Co. Ltd., 1952), pp. 175-179.

[2] Collingwood, *op. cit.* chap. 4.

is a proposition, while in so far as it gives rise to a further question, it is a presupposition. And if any presupposition "generates" a question but is not itself an answer to any prior question, it is an "absolute presupposition".

At first this view seems to be on a par with those previously mentioned, but closer inspection reveals an important difference. The import of the view now being considered is that statements as such are necessarily answers to questions and that questions as such are necessarily generated by logically prior statements. The intention of Collingwood's analysis is plainly that, at least in principle, the whole inquiry can be carried out quite definitively and scientifically.

It is at this point that I believe some of the characteristic errors and "blind spots" appear, on the part of those who would treat "presuppositions" systematically. I shall call attention to two of them.

(1) In the earlier part of this paper, I allowed that some or all of the inquiries there mentioned might be successfully executed. What I suspect is that Collingwood and others have failed to recognize sufficiently that *even if* these other things can be done, this provides little warrant for saying that what Collingwood here proposes can also be done.

It is not unusual to find philosophers making the mistakes of thinking that everything that can be done at all can be done completely and exactly, that anything which is intelligible within a specific context is just as intelligible without it, and that everything which has meaning has a clear and precise meaning. As F. P. Ramsey has said, "The chief danger to our philosophy, apart from laziness and woolliness, is *scholasticism*, the essence of which is treating what is vague as if it were precise and trying to fit it into an exact logical category".[1] Strawson appears to share some of Ramsey's view when he describes as a logical prejudice "the belief, or perhaps better, the wish, that if the uttering of a sentence by one person, at one time, at one place, results in a true statement, then the uttering of that sentence by any other person, at any other time, at any other place, results in a true statement".[2]

My point is simply this. It may be that X believes A on evidence B because he believes C and because believing C makes A plausible, given B. It does not follow from this that no proposition logically distinct from C might not serve the same office. Hence, the conjecture that there must be a set of absolute presuppositions $[C_1 \ldots C_n]$ of a whole body of similar beliefs $[A_1 \ldots A_n]$, based on evidence $[B_1 \ldots B_n]$ is far from plausible.

I do not believe that all those who use the term "presupposition" are talking nonsense. The kinds of things I mentioned in the first part of this paper make a considerable amount of sense, and other

[1] F. P. Ramsey, *The Foundations of Mathematics and Other Logical Essays* (London, Kegan Paul, 1931), p. 269.

[2] Strawson, *op. cit.* p. 176.

possibilities may make equally good or better sense. But one should guard against thinking that one can proceed as systematically and scientifically and definitively as Collingwood and others would have us believe. . . .

(2) There is a second and more serious confusion or error which vitiates much of the talk about presuppositions. One might trace it to the characteristic tendency among philosophers to "over-rationalize" human behaviour.

Collingwood's view about presupposing would seem to admit of two possible interpretations, on one of which it would be comparatively trivial and on the other of which it would be plainly false. If he means that for every statement a question *could* be found to which it would be the answer, and for every question a statement *could* be found which would generate this question, his view would be comparatively trivial. If, on the other hand, he means quite literally that every statement does in fact arise in answer to a specific and determinable question, and every question arises from a specific proposition or presupposition, his view seems to me to be plainly false. Yet as I understand Collingwood, he does have the latter in mind. If so, I submit that he, along with some other philosophers, makes the mistake of supposing that because a certain belief, statement, or question *can* give rise to one or another of these, therefore the latter *must* be generated by the former. On the whole question of the necessity and sufficiency of presuppositions, the "presuppositionists" are vague and ambivalent. I find that even Strawson, who is clearer than most, is more certain of what the relation of presupposing is *not* (namely, logical entailment) than he is about what the relation *is*.

Specifically, I submit that if and when one tries to discover why a certain belief, statement, or question arises, he may find that it is "because of" something of the same sort. But it may also be that what he will find is something quite different in the way of a "cause"—namely, something psychological, biological, or sociological.

Now of course one may object to this that the latter could also be what is meant by "presuppositions". I do not doubt that some philosophers *have* meant something of this sort. But what needs to be recognized is that the referent of this term "presupposition" is then no longer what I have taken it to be in this paper (*viz.* some kind of statement, belief, or question). One of the things which impress me is that many philosophers are either vague or confused about the kind of thing a presupposition is or else they play fast and loose with its meaning from one context to another. . . .

There are a number of topics or problems in connection with which the notion of "presuppositions" has been suggested. As already mentioned, Strawson suggests it in connection with the Aristotelian classification of statements. Others have suggested it in regard to the problem of induction. Still others have introduced

it in connection with ethical beliefs or statements. I suspect that if one pursued the notion in any of these areas, the ambiguities and difficulties I have suggested would emerge more clearly.

The area of ethics is a good case in point. Some philosophers have maintained that wherever there is an ethical belief or ethical judgment, there must also be an ethical principle or "presupposition" of the belief or judgment. They further specify that such an ethical principle must be something which the believer or judger consciously or unconsciously accepts. When it is then suggested to such philosophers that the belief or judgment is "generated" not by any ethical principle but by some biological, psychological, or sociological circumstance, they retort that this is not an "explanation" of the belief or judgment but rather an attempt to explain it away. The basis of such a retort is, it seems to me, that these philosophers are committed to accepting only one *kind* of explanation—*i.e.* one which involves some sort of *logical* connection between the belief or judgment and its basis.

There has been much discussion in ethics of "reasons" and of what constitutes a "good reason". Much of this discussion has been sound and valuable. But philosophers would do well to guard against conceiving "reasons" too narrowly. Few, if any, philosophers would deny or forget that man is a rational animal who has his reasons and his "presuppositions", but there are philosophers who show a decided tendency to deny or forget that man is or has anything else.

VERGIL H. DYKSTRA

University of Oregon

SELF-REFERENCE, TRUTH, AND PROVABILITY[1]

On the first page of his article, " ' True ' and ' Provable ' ",[2] Mr. L. Goddard asserts, " It is not clear to me what we are saying, in general, when we say that there is a sentence of a formal system which, although unprovable, is nevertheless true ". That this is in fact not clear to him is quite apparent from the rest of his article. Goddard seems to us to be confused about the notions of provability and truth, and also about the sense in which the formula constructed by Gödel is or is not " self-referring ".

We take up the last of these points first. He says of Gödel's formula that " it is peculiar . . . because it is self-referring ". But there are at least three different ways in which a sentence can be self-referring. One is that in which the sentence refers directly to one of its features as an inscription, either type or token. Such, for example, are the sentences ' The second word of this sentence has six letters ', and, ' This sentence is printed in red ink ', where in both cases ' this ' is taken to refer to the sentence in which it occurs. The first of these is true, the second, false. We do not see how paradoxes could arise from sentences of this kind.

It is when a sentence contains some semantical predicate, *e.g.* ' true ', that a paradox can arise. A formulation of the traditional Paradox of the Liar, such as, ' This sentence is not true ', where again ' this ' refers to the sentence in which it occurs, is of this kind. Goddard apparently thinks that it is this second kind of self-reference that is involved in Gödel's formula. He says that " roughly " it is :

The sentence with number g is not provable (1)

" where the sentence with number g is the one on the line above " (p. 14). Now strictly speaking, it is quite obvious that when Gödel's theorem is construed as a theorem about formalized *arithmetic*, then Gödel's formula is not (1). For, Gödel's formula is a formula of (we shall suppose) a formal system S of number theory, and therefore a concatenation of the primitive signs of S. The following list of signs is sufficient, and we may therefore suppose that Gödel's formula is a concatenation of these : $\forall, \exists, \sim, \&, 0, ', +, \times, (,), =, a, b, _1$. The variables of the system are a, b, a_1, b_1, a_{11}, b_{11}, ... Thus it is to be a formula just like the following :

$$\forall a \exists b\,(\sim(a + b = b + a') \,\&\, \sim (0 \times 0' = 0')) \qquad (2)$$

only much more complicated.

The Gödel formula can be interpreted to express a statement of ordinary arithmetic just as (2) expresses the statement :

> For every natural number x, there is a natural number y, such that neither is it the case that the sum of x and y is equal to the sum of y

[1] We wish to acknowledge our indebtedness to Professor Burton Dreben of Harvard University, who made many valuable suggestions concerning this paper.

[2] Mind, N.S. lxvii (1958), 13-31.

and the successor of x, nor is it the case that the product of zero and one is equal to one. (3)

This statement of arithmetic is obviously true. When we assert that Gödel's formula is true, we assert that it expresses a statement in the same way that (2) expresses (3), and that the statement which it expresses is true in exactly the same sense in which (3) is true.

In the usual proofs of Gödel's theorem,[1] it is shown, by a correlation of formulae with numbers, that the statement of arithmetic which Gödel's formula may be interpreted to express is true if and only if the formula is itself not provable in the system S. It is only in this derivative sense that the formula asserts anything about itself. It is clear that this weak sense of self-reference is quite different from the first two senses and does not, of itself, give rise to paradoxes.

In the proof of Gödel's theorem we *construct* a formula $A(b, a)$ of S, with two free variables b and a, such that (if Δ_r is the numeral representing in S the natural number r), for each r and s, $A(\Delta_r, \Delta_s)$ is a theorem of S if and only if r is the number of a proof of the formula with number s. Then, by the above-mentioned correlation of numbers with formulae, the statement expressed by the formula

$$\forall b \sim A(b, \Delta_n) \tag{4}$$

is true if and only if the formula with number n is unprovable, and therefore in the above sense says that the formula with number n is unprovable. This looks like (1). Thus it may be that Goddard thinks that Gödel finds an n such that the Gödel number of (4) is n, but this is in fact true for no n under the numberings that are used in the published proofs. The Gödel number of (4) is always greater than n.

In fact the undecidable formula obtained in the usual proofs of Gödel's theorem is not of the form (4) at all. It is constructed in three stages, as follows:

(i) We construct a formula $B(b, a)$ of S, with two free variables b, a, such that for every r and s, $B(\Delta_r, \Delta_s)$ is a theorem of S if and only if r is the number of a proof of the formula obtained from the formula with number s by substituting Δ_s for the free variable a wherever a occurs in the formula with number s. (In some cases, but not in all, $B(b, a)$ is obtained from $A(b, a)$ by substituting a term for the free variable a.)

(ii) We consider the formula

$$\forall b \sim B(b, a) \tag{5}$$

and calculate its number, say n.

(iii) Then the undecidable formula is obtained by substituting Δ_n for the free variable a in (5); *i.e.* it is

$$\forall b \sim B(b, \Delta_n). \tag{6}$$

The number of this formula is not n.

[1] *E.g.* S. C. Kleene, *Introduction to Metamathematics* (Princeton, N.J., 1952), sec. 42.

We have said that the Gödel formula (*i.e.* (6)) expresses a true arithmetic statement if and only if it is itself not provable in S. Gödel shows that if S is consistent, then (6) is unprovable in S, and it follows immediately that the formula expresses a true statement. There is nothing mysterious about this, and the law of the excluded middle is not used.

It is worthwhile to point out here that if the formula of S expressing Fermat's "last theorem":

> For every n greater than 2, there are no natural numbers x, y, z such that the sum of x^n and y^n is equal to z^n. (7)

were shown to be undecidable in S, then the theorem would have been shown to be *true*. For let F be the formula in question. If (7) is false, then there are numbers x, y, z, n, with n greater than 2, such that the formula F_1 expressing the statement that x^n plus y^n is equal to z^n is provable in S. But the negation of F can be deduced in S from F_1. Therefore if neither F nor its negation is provable in S, (7) is true.

Goddard's discussions of truth *in* a language and of whether Gödel's theorem implies that arithmetic is deductively incomplete are marred by hazy and shifting conceptions of provability and truth.

The logician would not ask the question of completeness about the *general* method (attributed speculatively by Goddard to Euclid) of assuming certain statements to be true and presenting "the reasons why certain other statements . . . (are) regarded as true" (p. 23). Given an exactly specified list of axioms and rules of inference, then the case of a true statement which must be added to the list of axioms is certainly a case where the system has been shown to be incomplete. Of course it must be *proved* that it *cannot* be deduced from the existing axioms by the existing rules of inference. Whether one wants to say that intuition has outstripped formalization here is a matter of choice. Certainly intuition (whatever it is that convinces us that the particular statement is true and not provable in the given system) has outstripped this particular formal system, but one could, as Goddard says, construct a new system which would include this statement as an axiom. However, in arithmetic there is another factor. This is that if we take *any* formal system which satisfies certain highly general conditions (including consistency) then a true formula can be produced which is not provable in the system. This fact certainly has "sting". For whatever the notion of number-theoretic truth is, one would have liked to capture it exactly by means of a single formal system, as can be done for quantificational validity or truth-functional tautology. Gödel's theorem shows that this cannot be done.

Let us try to give a more satisfactory account of what completeness is. There is a sense of completeness in which we say that a system is complete if the addition as an axiom of a formula not already provable results in an inconsistent system. Goddard says explicitly

that this is not what he means by deductive completeness, and he makes much of the fact that quantification theory is regarded as complete, though it is not complete in this syntactic sense. What Goddard has in mind is a concept of completeness which we could call semantic. It is this we have in mind when we say that a system is complete if every true formula is provable. What is required to explicate this notion is an explication of the notion of truth; the faults of Goddard's treatment of completeness, then, are the faults of his treatment of truth.

Throughout his paper Goddard wants to speak of a formula as true in a language rather than as true unconditionally, just as we speak of a formula as provable in a particular system rather than as provable unconditionally. However it is important to note that the respective relativities of the notions of truth and provability are quite different. Briefly, the notion of provability is relative to a specification of axioms and rules of inference for the system, while the notion of truth is relative to an interpretation of the system which is quite independent of the system's axioms and rules of inference. Goddard says that any definition of truth would consist of a set of assumptions and rules for applying them (p. 24). He considers this tantamount to setting up the axioms and rules of inference of a formal system. This is misleading because ordinarily in speaking of *formal* systems it is required that there be a class of configurations of formulae called *proofs* such that there is a mechanical way of testing whether a purported proof of a given formula really is one; in Goddard's terms, whether the rules have been applied correctly to assumptions which really are on the list. Furthermore, only notational features of the formulae involved are relevant to the determination whether a given configuration is a proof and therefore whether a given formula has a proof, *i.e.* is provable.

The interpretation by means of which truth is determined could, of course, be formally defined. Such a "definition of truth" would be of the type first given by Tarski.[1] It does not in general give a class of proofs satisfying the above conditions, and the question whether a system can be found which is complete with respect to a certain interpretation is equivalent to whether the specification of the interpretation does lead to a class of proofs satisfying the above conditions.

Goddard cheats by taking as an example the definition of validity in truth-functional logic, since it is well known that truth-functional validity can be decided by the mechanical procedure of truth tables. In the case of validity in quantification theory, whether some formal system will have as provable formulae exactly the valid formulae is a question which could only be answered by Gödel's proof of the

[1] "The Concept of Truth in Formalized Languages", *Logic, Semantics, Metamathematics*, tr. and ed. by J. H. Woodger (Oxford, 1956), chap. viii. For arithmetic, see also A. Mostowski, *Sentences Undecidable in Formalized Arithmetic* (Amsterdam, 1952), chap. iv.

completeness of quantification theory. The question whether some formal system yields all and only the true formulae of arithmetic is answered in the negative by the theorem of Gödel which is the subject of Goddard's paper.

These points are illustrated by the following example. Let S_0 be the system S of arithmetic which we have been discussing. For each $n = 0, 1, \ldots$ let S_{n+1} be obtained from S_n by constructing by Gödel's method an undecidable formula G_n of S_n and adding it as an axiom. For each n, we can say that G_n is true but not provable in S_n. There is a single definition of truth in terms of which each of these statements can be made. This definition would give the natural meaning of "true in arithmetic". Since it is independent of n, it certainly cannot be the same as provability in S_{n+1}.

Goddard arrives at the following definition of incompleteness:

> a formal system $\mathscr{L}$ is deductively incomplete if, and only if, there exists a consistent system $\mathscr{L}_1$ which (i) contains as theorems all of the theorems of $\mathscr{L}$ plus expressions which, though well-formed formulae of $\mathscr{L}$, are not theorems of $\mathscr{L}$ and (ii), is such that the additional theorems will add to the usefulness of $\mathscr{L}$ as determined by the initial purpose for which it was created (p. 29).

Our above considerations make clear that since a definition of truth is not the construction of a formal system, the question of completeness, when it is the question whether every true formula is provable, is not a question whether a new formal system can be constructed.

We do not, however, hold that our above considerations give a philosophical explication of mathematical truth. Rather, they describe formal properties of the notions in question, which any philosophical explication must explain rather than deny.

In the second clause of Goddard's definition he seems to take truth as a matter of usefulness. He disregards the ample reasons for regarding Gödel's formula as true and thinks that truth should be defined so that the formula is true only if it is useful, which he finds doubtful. We have made clear why the usefulness of the formula is irrelevant. However, although we sympathize with Goddard's doubt about the usefulness of the formula, we must point out that Gödel discovered the undecidability in S of another formula which is very useful indeed, namely the formula which expresses in the system the statement that S is consistent.[1]

CHARLES PARSONS
HERBERT R. KOHL

Harvard University

[1] Some of our discussion may bear on the treatment of Gödel's theorem in Appendix I of Part I of Wittgenstein's *Remarks On The Foundations of Mathematics* (Oxford, 1956).

A SIMPLER SOLUTION TO THE PARADOX OF OMNIPOTENCE

MR. MACKIE'S article "Evil and Omnipotence"[1] concludes by making the following claim:

> Quite apart from the problem of evil, the paradox of omnipotence has shown that God's omnipotence must in any case be restricted in one way or another, that unqualified omnipotence cannot be ascribed to any being that continues through time. And if God and his actions are not in time, can omnipotence, or power of any sort, be meaningfully ascribed to him?[2]

Without in any way wishing to support the case *for* God's omnipotence, I would like to present a very simple argument which seems to me to refute this claim.

The gist of Mr. Mackie's argument, as I understand it, is as follows. The question: can an omnipotent being make things which he cannot control? raises difficulties which can be resolved only by distinguishing between first-order omnipotence and second order omnipotence. Without such a distinction the question "cannot be answered satisfactorily either in the affirmative or in the negative"[3]. Given the distinction, on the other hand, "what the paradox shows is that we cannot consistently ascribe to any continuing being omnipotence in an inclusive sense"[4]. As I see it, however, the paradox can be resolved without recourse to this distinction, thus leaving the case for God's omnipotence as sound or unsound as it was before.

The paradox can be formulated as follows:

Either an omnipotent being can make things which he cannot control, or an omnipotent being cannot make things which he cannot control. If he can make such things then there is something which he cannot control; in which case an omnipotent being is not omnipotent. This is impossible. Therefore an omnipotent being cannot make things which he cannot control. Yet this means he is already not omnipotent; in which case, again, an omnipotent being is not omnipotent.

I do not wish to challenge the first step in the argument, namely, that if a being can make things which he cannot control, then he is not omnipotent. I do, however, dispute the second. I deny, in short, that from the statement, "X cannot make things which X cannot control" it follows that X's capacity for making things, is limited. For this statement can be reworded without alteration of meaning, in any of the following ways:

"There is nothing of which it is true both that X can make it and that X cannot control it",

[1] MIND, lxiv, no. 254 (April 1955).
[2] *Op. cit.* p. 212.
[3] *Op. cit.* p. 210.
[4] *Op. cit.* p. 212.

" It is not the case that there is anything such that X can make it and X cannot control it ",

" If X can make anything, X can control it ",

" Everything X can make, X can control ".

But, clearly, from the fact that everything X can make, X can control, nothing follows as to the omnipotence, or otherwise of X. Thus, if my argument is correct, the paradox (so-called) does not, in fact, show that " unqualified omnipotence cannot be ascribed to any being that continues through time ". If anything, it adds support to the case for God's omnipotence by showing that an omnipotent being can control anything he can make.

I do not say that some Paradox of Omnipotence similar to the one considered, might not be formulated, that we could not get a paradox out of God's omnipotence however hard we tried. But I do deny that, as formulated, the paradox can be resolved only in an indirect way which leaves God's omnipotence impaired.

In conclusion, it is interesting to note that we have here, a case in which the logicians' analysis of the form of a universal conditional statement, plays a useful non-technical role. I am, in fact, resting my case upon it. Nor am I, as far as I can see, laying myself open to the frequently heard objection : it's no use trying to impose the straight-jacket of logical symbolism upon ordinary language. Those who object to such an analysis in any particular instance have, perforce, to produce a more acceptable one. But in this instance it is not easy to see what alternative there could be.

University College of North Staffordshire G. B. KEENE

MRS. FOOT ON MORAL ARGUMENTS

In an interesting article in Mind (lxvii, no. 268 (October, 1958), 502-513) Mrs. Philippa Foot offers an illuminating discussion of the nature of moral arguments, and especially of the crucial distinction between description and evaluation.

Mrs. Foot quite correctly, I believe, points out that the distinction between the descriptive and the evaluative cannot, in the form in which G. E. Moore tried to do it, be based on the distinction between two kinds of qualities in *things*, *viz.* as a concern with natural and non-natural properties, respectively. Mrs. Foot also correctly draws attention to the difficulties involved in basing this distinction on differences between two kinds of *terms* or propositions, words or statements, *viz.* that description is formulated in 'factual' statements and couched in 'factual' terms, whereas evaluation expresses itself in 'emotive' judgements by means of 'emotive' terms; in the ordinary sense of 'descriptive', Mrs. Foot observes, the word 'good' is used as a descriptive term. Apart from the fact that Mrs. Foot here neglects the obvious differences that there are between words like 'square', 'comfortable', 'good' by calling them all 'descriptive' she is right in attacking the wholesale classification of words into comprehensive classes. Perhaps it is more correct to say that a word like 'good' may sometimes be cognitive, descriptive or explicative, sometimes evaluative; on the other hand, a child may quite well feel it as a negative evaluation if it is pointed out to it that the wheels of the car it has just drawn are 'square'. This shows that the classification should be not of *terms* simply but rather of the particular uses and functions of terms. The distinction between the 'descriptive' and the 'evaluative' will then be seen as two kinds of *activities* based on differences in their purpose.

However, Mrs. Foot does not extend her discussion to such considerations. For the sake of argument she admits the distinction between two kinds of statements: (1) statements of factual evidence, and (2) statements of moral judgements; between (1) factual premises and (2) evaluative conclusions. What she wants to investigate is the relation between (1) and (2), to question "the thought that there is no logical connection between [1] statements of fact and [2] statements of value", this "thought" being one of the "assumptions" leading to the conclusion that moral arguments may always break down even when there is agreement about the factual premises. Mrs. Foot attempts to demonstrate, or at least suggests, that it is possible to have "an example of [1] a non-evaluative premise from which [2] an evaluative conclusion can be deduced".

Her central argument is built exclusively on one example, and it has therefore to be considered carefully. Taking her point of departure in the evaluative word 'rude' used in such a statement as 'This behaviour is rude' (R), Mrs. Foot asks for the factual conditions which, if fulfilled, would count as 'evidence' for the statement

R: "The right account of the situation in which it is correct to say that a piece of behaviour is rude, is, I think, that this kind of behaviour causes offence by indicating lack of respect." Mrs. Foot suggests that a person who grants that these conditions of offence are fulfilled (proposition O) cannot deny that the behaviour is rude (proposition R). Since proposition O is a description of a state of affairs, we have here an example of a descriptive proposition entailing an evaluative judgement. I think this argument is fallacious and that it is based on an ambiguity as to the nature of the premise posed as non-evaluative by Mrs. Foot.

To begin with: What exactly are the factual conditions the fulfilment of which would justify R, according to Mrs. Foot? Her argument seems to suggest that they consist in a certain kind of offence being actually caused, offence at behaviour indicating lack of respect. On this account a person cannot call a piece of behaviour rude unless he knows that it has actually caused such offence, whereas such an evaluation must necessarily follow once it has been made clear that such offence has been caused. Now, suppose a man from Luna (where the respect for women, particularly for old women, is supposed to be very low—wherefore they are called Lunatics) visits England and sees an old woman being pushed into the street by a young fellow. The scene which follows leaves no doubt that the woman is offended exactly in the way required. Would the Lunatic say 'What a rude fellow!'? Hardly. Mrs. Foot, accompanying him, would indeed stare at him and ask: 'But, for Heaven's sake, what kind of behaviour *do* you consider rude?' A perfectly legitimate answer for the Lunatic to give would be: 'Of course, if the person being pushed had been a *man*, that would have been a very rude thing to do.' The mere causing of such offence is thus not a *sufficient* condition for calling a piece of behaviour rude, and a similar thought experiment ('Whether or not you feel offended, I think he was very rude towards you!') may show that we do not even consider actual offence a *necessary* condition for judging some behaviour rude. True, from 'He is (was) offended by that behaviour' follows 'He finds (found) that behaviour rude', from 'I am offended by this behaviour' follows 'I find this behaviour rude', from 'It is a fact that I am (was) offended' follows 'It is a fact that I consider(ed) him rude', but in each of these three pairs of statements there is no movement from descriptive to evaluative propositions; the first and the third pair describe attitudes, the second pair expresses the having of an attitude. Mrs. Foot's argument seems convincing only by a basic ambiguity, for when the two statements are formulated as 'It caused offence' and 'It is rude' without specifying *to whom* and *for whom*, the distinction between *describing* an attitude and *expressing* an attitude becomes blurred, and offensiveness and moral evaluation are viewed as a public affair only, although it is always particular persons who are offended and although moral evaluation does not occur except in the minds (or hearts) of particular men.

Considerations similar to my Lunatic example may have urged Mrs. Foot to add an important reservation to her attempt at stating the factual conditions on which moral evaluation may rest: "It may be that in attempting to state the rules which govern the assertion of moral propositions we shall legislate against a moral system radically opposed to our own. But this is only to say that we may make a mistake. The remedy is to look more carefully at the rules of evidence, not to assume that there cannot be any at all." Let us follow her advice and look more carefully at the conditions which would have to obtain in order to justify a moral judgement of 'rudeness'.

Since the actual occurrence of offence seems to be neither a necessary nor a sufficient condition, a more cautious phrasing may be proposed: 'A certain kind of overt behaviour indicating lack of respect and thus involving the possibility of causing offence.' Perhaps we should have to add one more condition: 'assuming an intention in the agent to show lack of respect.' All these conditions may perhaps be ascertained and stated by factual investigations and in factual statements. It is obvious, however, that we would have to specify also towards whom lack of respect has to be shown. If lack of respect is shown towards a dog or towards a thief in the act of stealing, 'rudeness' is perhaps not the term to use. A full statement of all the factual conditions—the kinds of persons and the kinds of circumstances—implying 'rudeness' would be an infinitely long list. We are faced with the same kind of difficulty which the phenomenalist encounters in trying to exhaust the content of a physical object statement by means of sense-data statements, and for the same reason—by attempting to translate statements in one category into statements in some other category, and expecting equivalence. In fact, the list can only be exhaustively stated by some such formula as this: 'toward persons whom the agent *ought to* respect', *i.e.* by a formula involving some kind of evaluative term, or by specifying the behaviour as indicating lack of *due* respect.

Such a formulation of the conditions under which 'rudeness' may properly be applied or of the rule governing the proper use of 'rude' ('A certain kind of behaviour involving the possibility of causing offence in virtue of an assumed intention (or habit) in the agent to show lack of respect towards persons whom he ought to respect') may satisfy the Englishman, the Chinese and the Lunatic, but the conditions enumerated are no longer factual. Or, rather, to the extent that the conditions are considered purely factual, they do not force the observer to adopt any specific moral attitude on the basis of them. The Lunatic may perfectly well realize a situation where these conditions are fulfilled: there is the relevant kind of behaviour (say, pushing) that may well cause offence; the circumstances may make it clear that the agent did not behave accidentally in this way; he may even have learned from Mrs. Foot that in England such behaviour is considered disrespectful. On the strength of all this evidence, the Lunatic—polite as he is—may even say: 'What a

"rude" person!' but as long as he has not himself embraced the moral rules which he realizes as applying in English society, 'rude' will be in quotes—a familiar way of indicating that a word usually applied in one category is now used in some other category; in the present case 'rude' is a factual, descriptive term functioning in a sociological observation, for it does not express the moral attitude of the Lunatic. However, once the Lunatic embraces the English rules of proper behaviour, the quotes disappear—but why? Is it because the factual requirements are differently conceived? Is it because a different factual state of affairs is seen to obtain? No, exactly the same factual situation may now give rise to a genuine moral judgement. The essential difference is that the content of the 'ought'-clause of the rule governing the application of 'rude' as an evaluative term is now no longer derived and considered cognitively, in the same way as an anthropologist may describe the oughts and duties of some primitive people; the content is now determined habitually, thus functioning as a rule for the evaluation of behaviour, and as such it does not go into the factual description of the situation but expresses the attitude of the moral judge.

My formulation of the conditions under which some behaviour may properly be called 'rude' may be faulty, but it seems that any attempt to state the rules governing the assertion of moral propositions will show the necessity of including some evaluative clause, and that, if such a clause is missing, the conditions formulated will be found not to entail any moral judgement. The question of the 'evidence' required for the assertion of moral judgements is quite another matter, for it is obvious that some—perhaps all—moral terms require for their proper use the fulfilment of factual conditions of some specifiable kind. But the important point is that, although such conditions—*e.g.* a certain kind of behaviour indicating lack of respect, thus involving the possibility of causing offence—may be necessary conditions for the use of moral terms and the assertion of moral judgements, they never seem sufficient, and their statement therefore does not point out the essential requirements which have to be fulfilled in order to arrive at some specific moral evaluation. These requirements may perhaps more properly be called 'justifications' than 'evidence'.

ANFINN STIGEN

University of Oslo

A CRITICISM OF ROSS'S HYPOTHETICAL 'I CAN'

In the *Foundations of Ethics*, Sir David Ross gave up the doctrine which he formerly held that our duty is to do the 'objectively right' act. His later and presumably present position is that man is duty-bound only to the 'subjectively right' act, *i.e.* to "The self-exertion which he *thinks* to be morally most suitable in the circumstances as he takes them to be".[1] Kant's principle that 'I ought' implies 'I can' provides Ross with grounds for rejecting the idea that it is our duty to do the 'objectively right' act, since only omniscience could know what that act is. Not being omniscient, we cannot know it; not knowing it, we cannot do it; and since we are not able to do it, it cannot be our duty to do it. According to Ross, one idea is "inseparable from the thought of duty". "This is the thought that anything that we ought to do must be something that we not only can do, but can do with the knowledge or at least the opinion that it is our duty."[2]

This seems to be an eminently reasonable position, and in part we can accept it. (1) First, we accept Ross's final conclusion that the subjectively right act is the one that in fact has a claim upon us. (2) We also accept, in a sense, his concession that this is true because we are in fact able to do the subjectively right act. But we shall point out that some of Ross's contentions are inconsistent with this second point. We shall argue that if they are true even the subjectively right would not be our duty, indeed that we should have no duties at all if we did not want to have them. The basic point of this criticism is that if Ross is correct in stating and defending his position as a determinist, his concern with the meaning of the word 'right' is misplaced. If the conclusions which he draws in his chapter on freedom in the *Foundations of Ethics* are true, then his analysis of the meaning of 'duty' both in *The Right and the Good* and the *Foundations* is false, or that the conclusions on freedom are false and the analysis of duty is true.

A. In discussing the relationship between the 'subjective' and the 'objective' right, Ross appeared to defend the position that we are in fact able to do the subjectively right act categorically. We ought not to do the objectively right act because we cannot, because conditions always forbid the doing of such acts knowingly. However, when Ross begins to discuss the 'I can' in relation to the subjectively right, it turns out that what he offers us is not a categorical but only a hypothetical 'I can'.

> The general formula must hold good, that 'I can do this' is really a hypothetical proposition—'if a certain condition is added to conditions already present in me, I shall do so-and-so'. And it is easy to see what the additional condition must be. It consists of my wishing

[1] Sir David Ross, *Foundations of Ethics*, p. 161.
[2] *Ibid.* p. 163.

to do this. 'I can do this' means 'I have such a nature that if I want to perform the activity of setting myself to do this, I shall perform it'; and 'I can refrain from doing this' means 'I have such a nature that if I want to refrain from doing this, I shall refrain from doing it'. Or, putting it briefly, 'I can do this or that' means 'I shall do this if I want, and I shall do that if I want'—'want' being here a brachylogy for 'want predominantly'.[1]

Now, it appears to be very clear that the hypothetical 'I can' which Ross is here discussing is not the kind of 'I can' under which men ordinarily think themselves to be obligated. They would certainly think it odd to say, 'I can if I must', *i.e.* if psychological conditions make it necessary. In the first place, the statement is redundant. If I must, *i.e.* if conditions make it necessary, of course I can. But the difficulty is that Kant's principle stated as "'I ought' implies 'I must'" makes no sense whatsoever. Yet, this is the only formulation of it which Ross can legitimately make. That 'I can' means only 'I must' in Ross's system is seen in his words: "Now if in the end I do the right act, then, if the defence of Determinism which I have put forward is sound, my doing the right act implies that, just before I did it, conditions which made it necessary for me to do it were present, and similarly my doing the wrong act, if I do *that*, implies that conditions which made it necessary to do that act were present."[2]

The point at which I am driving here comes out very clearly in the following case. Ross seems to admit that it is possible to know what a subjectively right act would be and at the same time fail to do that subjectively right act owing to the predominance of some other motive, *e.g.* the motive to do something which is not my duty. Introspection clearly shows that this is the case. (Ross argues, contrary to Kant, that the desire to do one's duty is a motive among motives.)[3]

If 'I ought' implies a categorical 'I can' as here maintained, it also ought to follow from Ross's position that I am under obligation to do a certain act only when I believe that that act is my duty and at the same time have the 'predominating desire' to do that duty—only then 'can I' do it. Correspondingly, it ought to follow that when I believe that an act is my duty and at the same time I do not have a 'predominating desire' to do my duty I am mistaken in thinking at all that the act is my duty, since I clearly cannot do it. The man who is tied to a tree on the bank while his best friend drowns in a nearby stream is under no obligation, either objectively or subjectively, to save him, since he cannot. Similarly, the man who is tied to his passions while his sense of duty perishes in the deep is under no obligation to act dutifully, since he cannot. If he were omniscient he could perhaps do the objectively right act; if he were free from his passions he could perhaps do the subjectively right act; but he is not, he cannot, he ought not.

[1] Ross, *op. cit.* pp. 240, 241. [2] *Ibid.* p. 235. [3] *Ibid.* p. 228.

Ross then is faced with the following dilemma : either the only conditions under which I am under obligation are those under which I most want to be under subjective obligation ; or the notion of subjective obligation must be rejected *in toto*, and it must be admitted that the conditions under which we are in fact obligated never occur at all. It is hardly to be expected that Ross will take the second alternative, since the situation does arise in which a man both thinks that he has a duty and also has a ' predominating desire ' that will effect the doing of that duty. For this reason Ross has some escape from the following criticism of H. D. Lewis : " Ross's argument thus proves too much for his theory. For if he is to argue that ' ought ' has no application to motives because ' we cannot desire a certain end at will ' it seems to follow also that ought has no application to conduct in any regard." [1] Still, there seems to be no escape for Ross from the position that we are under obligation only when we most want to be under obligation. If this is true, then the man who can successfully resist the temptation to act from a sense of duty or from a motive that will effect a dutiful act throughout his whole lifetime may be said never to have had an obligation to do anything whatsoever. This is certainly a paradoxical conclusion to say the least. In my opinion it is completely unacceptable. Ross can avoid it only by abandoning his determinism and solving the problem of freedom.

B. Ross maintains that the hypothetical ' I can ' is justified on its own merits (but we have argued that this is not true). Believing that it is justified on its own merits, he goes on to argue that it is also justified for its " practical utility ". We must conclude, since Ross has failed to justify the hypothetical ' I can ' on other grounds, that to the person who is aware of this theoretical failure even its practical utility is nullified. Ross writes as follows :

> Consider the effect of holding the opposite belief. Suppose I believed that I could not do my duty ; then I should be thinking, in effect, this : ' Though the act is the right one in the circumstances, and one which a better man ought to do, yet since I can't do it there is no use in my trying to do it.' I should resign the struggle, and do the act which most appealed to me on other grounds. But if I keep before me the thought ' that is the right thing to do, and I don't know of anything that makes it impossible for me to do it, and do know of something, *viz.* the sense of duty in me, which makes it appreciably probable that I shall do it ', the wish to do my duty is kept alive and allowed to have its full weight in determining my action.[2]

About this argument, two things are to be noted. First, it assumes that I do in fact want to keep my sense of duty alive. If this were true, however, to the degree sufficient on Ross's theory to produce that result, I would not have to resort to these roundabout techniques to do so ; and if this were not true, I would never take the trouble to follow out his suggestion anyway. Next, the argument

[1] Hospers and Sellers, *Readings in Ethical Theory*, p. 584.
[2] Ross, *op. cit.* p. 238.

assumes that even though I know (according to the analysis of the preceding paragraph) that I have no duties which I do not already most strongly want to have, I shall nonetheless be able to close my eyes on this fact and assume that I do in fact have duties, regardless of the strength of my wants. That is, the argument assumes that the suggested technique could be effective. This situation is analogous to the case of the man who postulates the existence of God merely on the grounds that it would give him comfort if it were so. As Broad points out in his *Five Types of Ethical Theory*, "You would not get much comfort from postulating the existence of God so long as you remember that you were postulating it only in order to give yourself comfort".[1] Similarly, the man who postulates duty will hardly feel bound by it when he remembers that it is a mere hypothesis that may be contrary to fact. To the man who knows, duty will have no place in his personal ethic unless he so desires, if Ross is correct. To be sure, it might still be maintained that "duty" may be an effective means of social control as long as men can be kept in ignorance of the fact that they have no duties unless they want to have them. But in that case, those who know should not tell!

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CAN DOUBT BE DOUBTED?

THE question can be interpreted in two different ways. It may mean: (i) is it possible psychologically to doubt an act of doubting?; or, (ii) can an act of doubting or its result be logically doubted? Here we shall be concerned mainly with the second question.

The second question can be elaborated thus. Let p be any proposition whatever not containing 'doubt'; let us doubt it and express the result as 'dp' meaning "'p' is doubted" or rather, 'it is doubtful that p'. (The difference between what is actually doubted and what is doubtful will make no difference to our argument.) Now the question is whether dp can again be doubted, or symbolically, whether d(dp) is possible or not. If we symbolise the class of doubts about p as 'd_1', the class of doubts about doubts about p will be d_2. The first class of doubt (d_1) that we have in dp is of the first order; d_2 is the class of doubts of the second order and corresponds to d(dp), and so on. Now in answer to the question whether d(dp) is possible or not, we find that there is no difficulty, logical or psychological, in giving an affirmative answer to it. There is no logical difficulty, for to assert 'it is doubtful that it is doubtful that p' is not to involve us in any self-contradiction. There is no psychological difficulty either. For, although ordinarily we do not need to doubt a doubt (just as ordinarily we do not need to know that we know) we do doubt whether the characterisation of a mental state as doubt is correct or not when the occasion arises, and we can always do it if we so like. As a matter of fact, the process of doubting a doubt can be extended indefinitely, so that we get an unending series of classes of doubts, the first order doubts, the second order doubts about the first order doubts, then the third order doubts about the second order doubts, and so on *ad infinitum*. So we can say that every doubt, no matter to what order it belongs, can be subjected to a doubt of a higher order; *i.e.* for every d_n we can have a d_{n+1}, however great n may be.

But this goes directly against the Cartesian doctrine that doubt cannot be doubted. To see why the Cartesians hold it let us analyse their argument. They assert that whenever a doubt is doubted, we are necessarily left with a doubt. For let us say that a doubt is doubted; but this can be done only by another doubt, and this doubt is asserted. So doubt can be doubted only through the assertion of doubt.

Now, in criticism of this argument, we can point out that it rests entirely on the assumption of the identity of the doubt doubted and the doubt doubting it, an assumption which the argument does not justify or even state. To doubt a doubt is to be still left with *a* doubt, but unless the two doubts are identified we cannot say that the assertion of the second is the assertion of the first. Now this identification is effected by Descartes in his concept of a universal doubt. The doubt which is doubted may differ from the doubt that doubts it, but they differ only as *particular* doubts, but as belonging

to the same class of doubts they are identical, *i.e.* they agree in being doubt. This universal doubt cannot be doubted, for this includes not merely all doubts about objects, the doubts about these doubts, and so on, but also all doubts about *itself*. This is the reason why if we try to doubt it it gets asserted.

Thus the Cartesian argument proves the indubitability of only this doubt, but not of any other, and if we do not admit this universal doubt there remains no indubitable doubt, and it is not self-evident that such a doubt must exist. So what Descartes has really proved is this: *If* there is such a universal doubt then it is indubitable. But really we can go farther and say that Descartes has not been able to prove even this. For the concept of the universal doubt is obviously an instance of the 'liar paradox' and is logically unjustifiable. There is no class of doubt which includes itself as its own member. A doubt can be doubted only by a doubt belonging to a higher order. The doubts of the different orders cannot be brought under one universal doubt. If we do bring them under one universal doubt, we do not prove that it is indubitable, but simply that it is an unwarranted assumption. To prove the paradoxical nature, and therefore the illogicality, of this concept we can point out that even if such a universal doubt is assumed, it will not prove Descartes's point, for it cannot prove anything. Descartes argues that if we doubt this universal doubt which includes its own doubt, then it gets asserted; so it must be asserted, *i.e.* it is indubitable. But we can prove exactly the opposite also. For to assert such a doubt is also to doubt it. If the doubt that is doubted and the doubt that doubts it are identified, then just as the first doubt doubted through the assertion of the second doubt gets itself asserted, so also the second doubt in doubting the first doubts itself. The assertion of a universal doubt which includes its own doubts is also the assertion of its own doubts. Thus we see that even if the concept of the universal doubt is brought in, it does not help Descartes in asserting that it is indubitable. For although the universal doubt cannot be subjected to any external doubt because it is all-comprehensive and there is no other doubt left outside it, it remains, for that very reason, subjected to itself. To be left with a universal doubt is still to be left with a certainty, *viz.* of the universal doubt itself. But Descartes cannot have even this certainty because his universal doubt is a *doubtful* doubt; to assert its existence is to make it doubtful. So Descartes's dictum that (the universal) doubt is indubitable cannot even mean that if there is a universal doubt then it is indubitable; it can mean only that if there is any such universal doubt then it is indubitably a universal doubt. But as to say 'A is indubitably A' is not to say anything more than 'A is A', what Descartes has succeeded in proving is simply 'the universal doubt is the universal doubt', where the 'is' has no existential import at all.

An attempt may be made to justify the concept of a doubt doubting itself on metaphysical and epistemological grounds. It may be

argued that because every knowledge must know itself (otherwise knowledge cannot be known at all), so every doubt must doubt itself, otherwise it cannot be doubted at all. In criticism of this argument we can point out that even if we admit for the sake of argument that every knowledge must know itself,[1] it does not follow that every doubt must doubt itself.[2] Otherwise we should be able to argue with perfect justification that every inference must infer itself, every perception must perceive itself, etc., which is, of course, false.

An attempt may be made to prove logically that doubt cannot be further doubted. It may be pointed out that though Descartes tried to prove it only by assuming a universal doubt which cannot be logically justified, such an assumption is not at all necessary. That doubt cannot be doubted can be proved simply by showing that d(dp) is equivalent to dp, and this can be done in the following way. We doubt that p only when we have both the alternatives p and not-p, *i.e.* ' dp ' can be translated into ' either p or not-p ', in symbols, ' ApNp '[3]. Now to doubt dp, *i.e.* to have d(dp), is AApNpNApNp. But as NApNp, which is KpNp, is a manifest contradiction, the whole AApNp NApNp reduces simply to ApNp, for any contradiction can be dropped as an alternative. So we find that d(dp) is simply dp, so that every attempt to doubt a doubt must fail.

Against this proof the obvious reply is that ' dp ' cannot be translated as ' ApNp '. For, firstly, ApNp is a tautology which is always true, whereas dp is far from being a tautology. Secondly, while A and N are truth-operators, d is not. To know the truth-value of Np, for example, it is sufficient to know the truth-value of p, but the truth-value of dp cannot be determined by simply knowing the truth-value of p. So ' dp ' cannot be translated as ' ApNp '.

Another logical interpretation of doubt which avoids these objections but does not support Descartes's argument can be given with the help of modal logic. It may be argued that when we doubt that p, we simply assert that not-p is possible ; *i.e.* ' dp ' can be translated as ' MNp '. Now to doubt dp is to have MNMNp. But as NMN is the impossibility of falsity, it is simply necessity. So MNMNp reduces to MLp. Now we may hold that what is possibly necessary is actually necessary so that MLp reduces to Lp. Thus to doubt a doubt is to destroy it ; to assert that a proposition is doubtfully doubtful is to assert that it is necessary ; ' it is doubtful that it is doubtful that p ' simply means " ' p ' is necessary ". So d(dp) does not reduce to dp, therefore they cannot be identified. Now if we try to doubt again this d(dp), we get MNLp. As NL is MN, MNLp be-

[1] For a detailed criticism of this theory, see Professor Ryle's *The Concept of Mind*.

[2] That if knowledge knows itself still it does not know itself indubitably *as knowledge*, can be shown, but it is beyond the scope of the present paper.

[3] With the exception of ' d ' (with and without subscript) the notation is the Polish notation as used by Professor A. N. Prior in his *Formal Logic*.

comes MMNp. But as what is possibly possible is simply possible, MMNp reduces to MNp, and we are back at the original doubt.

Now even if we accept this argument Descartes's argument remains refuted. For though the doubt of the doubt of a doubt may be identical with the original doubt, no doubt of doubt can be so. As a matter of fact, the operator d, understood in this way, behaves like the truth-operator N. By negating an affirmative proposition any odd number of times, we get a negative proposition, but by negating it any even number of times we get an affirmative proposition. But this does not prove that any proposition can be identical with its own negation, or that no proposition can be negated. When we doubt a doubt an odd number of times we get a doubt, but if we doubt a doubt an even number of times we are left with a certainty. But this does not prove that a doubt cannot be doubted. The assertion of d_2 cannot be the assertion of d_1. Only our theory of the order of doubts has to be rejected. For we can no longer hold that a doubt can be doubted only by a doubt belonging to a higher order, for if we doubt the doubt of a doubt, we do not have a doubt of a higher order, but we are back at the doubt of the first order. But there is really no necessity to accept this position. For, firstly, the principle by which MLp has been reduced to Lp is itself very doubtful, and the system of modal logic in which it occurs as an axiom is, though not self-contradictory, not usually accepted by logicians (*cf.* Prior, *ibid.* pp. 200-207). Secondly, we may reject the interpretation of 'dp' as 'MNp'. For we may point out that to doubt that p is to say not that not-p is possible, but that not-p, *too*, is possible; that is, to doubt that p is to assert that both p and not-p are possible. Hence 'dp' has to be translated not as mere 'MNp', but rather as 'KMpMNp'. But now it is obvious that neither d(dp) nor d(d(dp)), nor any other doubt can be identified with dp. For there is no way of reducing KMKMpMNpMNKMpMNp, the d(dp), to KMpMNp, the dp. So we get here an unending hierarchy of doubts.

There is, of course, nothing which can compel us to accept this conclusion either. But then the theory of the order of doubts is not needed to refute the Cartesian argument; it is sufficient if we distinguish d_2 from d_1 in such a way that an assertion of d_2 cannot be an assertion of d_1. And what we hope to have shown is that it is such a distinction between them which is logically more justified than their identity.

SIBAJIBAN

Saugor University

REGRESS ARGUMENTS IN PLATO

THERE are regress arguments in three Platonic dialogues; two in the *Parmenides* (131e-132b; 132d-133a) and one each in the *Republic* (597c) and the *Timaeus* (31a-b). It has been argued, notably by Cornford,[1] that the *Parmenides* regresses are sophistical arguments against the Forms and deliberately involve a fallacy. "The arguments of the *Republic* and the *Timaeus* indicate that Plato was not blind to the fallacy in Parmenides' assumption that largeness is a large thing." A view somewhat similar to this occurs in Cherniss.[2] I wish to argue (*a*) that the *Republic* argument contains (whether or not self-consciously) Parmenides' assumption and so the *Parmenides* regresses are a valid critique of it; (*b*) that the *Timaeus* regress can be almost completely assimilated to the *Republic* argument; (*c*) that these facts provide support for G. E. L. Owen's[3] thesis that the *Timaeus* is earlier than the *Parmenides*.

(*a*) The *Republic* argument is designed to prove that God made but one Form of Bed. Had he made two, then yet another would have appeared behind them accounting for what the pair have in common. God realized this, and wishing not to be the maker merely of a particular bed, he made but one, which was then the Form of Bed. One of Plato's premisses here is, clearly, ἐὺ ἐπὶ πολλῶν and this premiss yields the first step in this argument. It is not easy to be sure precisely what this premiss amounts to, but I think it is perfectly safe to assume that it will not yield the remainder of the conclusions of this reductive argument. For from the fact that a third thing arises behind the pair, it does not follow either (i) that the pair (or one of them) does not account for what is common to sublunary beds, or (ii) that the pair thereby become particular beds or (iii) what is implicit here and explicit in the *Timaeus*, that what arises behind the pair will be an entity which accounts for what sublunary beds have in common.

For these conclusions to follow, we must adopt the premiss that what God constructs is itself a bed, *i.e.* that when God sets out to make a Form, he makes something which *has* the character for which the Form accounts. This premiss of self-predication is not infrequent in the earlier and middle dialogues as Owen and Vlastos[4] have pointed out. But this premiss is just what Parmenides assumes. And given this assumption, what ought to follow is not the finite harmless regress of the *Republic*, which is valid only as a first step, but rather the infinite regresses of the *Parmenides*. And—I stress

[1] F. M. Cornford, *Plato and Parmenides*, p. 90.

[2] H. Cherniss *Aristotle's Criticisms of Plato's Academy*, pp. 292-298.

[3] G. E. L. Owen, "The place of the *Timaeus* in Plato's dialogues", *Classical Quarterly* (1954).

[4] G. Vlastos, "The Third Man Argument", *Philosophical Review*, 63 (1954), 336-339.

it again—without Parmenides' assumption, the *Republic* argument cannot establish conclusions (i), (ii), and (iii) above. It is clear therefore, that the *Parmenides* regresses can be regarded as a valid and indeed an elegant critique of the earlier argument and therefore of the paradigm theory of Forms, whether or not Plato so regarded it.

(*b*) The *Timaeus* argument is *prima facie* the analogue of the *Republic* argument, but we must notice divergences. What is to be proved is that the Demiurge created but one sublunary intelligible living creature (*i.e.* the World). He created but one so that it should be as like as possible to the Form in the image of which he made it. That Form must be unique for "that which embraces all the living creatures that there are, cannot be one of a pair; for then there would have to be yet another Living Creature embracing those two, and they would be parts of it, and thus our world would be more truly described as a likeness, not of them, but of that other which would embrace them".[1]

Here again the ἐν ἐπὶ πολλῶν premiss, by itself, fails to establish (i) that the pair (or one of them) does not account for the F-ness which is common to sublunary F-things; (ii) what is probably implicit here in (i) and explicit in the *Republic*, that the pair thereby become particular F things; (iii) that what accounts for the quality which the pair have in common also accounts for what sublunary F-things have in common. However, all three conclusions are yielded in the first step if we attach a self-predication premiss. I think it obvious that conclusion (iii), which is the most important of them, could not possibly be established unless Parmenides' assumption is among the premisses of the argument. For if it is not the case that each member of this pair (failing to be the Form of F *only* in that it is not unique) also *has* the quality F-ness, then what embraces those two would not be something which accounts for the *F-ness* of sublunary things, but for some different quality which the pair do share. And so the major conclusion of the argument would simply not follow.

There are differences between the *Republic* and *Timaeus* arguments. The latter seeks to establish the uniqueness of the World by establishing the uniqueness of the Form, but the Form's uniqueness is established by means of the same finite regress. In the *Timaeus*, the relation between Form and instance is construed as that of whole to part; as a sort of embracing. But it surely has to be conceded that this is a kind of the original-and-copy relation which occurs elsewhere in the dialogue, and at this point in it too. And although this relation is asymmetrical it includes the symmetrical relation of similarity.[2] But this would also yield the second regress of the *Parmenides* if not the first. Therefore, we may safely assimilate the *Timaeus* and *Republic* arguments with only minor qualifications.

[1] F. M. Cornford, *Plato's Cosmology*, p. 41-42.
[2] W. F. R. Hardie, *A Study of Plato*, p. 88 ff.

(c) Assuming that these arguments are correct, we may say that the *Parmenides* regresses can be regarded as valid and illuminating critiques of the *Republic* and *Timaeus* regresses. However it is another matter to conclude that Plato must have realized this and so that the *Parmenides* must be the latest of the three dialogues. What I have argued does not conclusively prove this. But if we do not accept it, we must conclude instead that although, as is generally recognized, the three arguments are strongly analogous, and though it is quite plain that the *Parmenides* regresses are valid, and also fairly clear what their premisses are, Plato himself used the arguments with only accidental validity, and on no one of the three occasions did he see their point even though on one of these occasions, the point is clearly and validly made. Plato may have made the error. Certainly there are bad arguments elsewhere in the dialogues. But nowhere else does he make so crass a blunder as to get an argument right, not to see that this has happened, and then to use the argument wrongly later. One feels great misgivings in accusing Plato of such blindness to his own achievements. One might, instead, accuse Plato of being disingenuous in the *Timaeus*, using an argument he knew to be invalid. But surely it is less paradoxical to suppose the *Parmenides* to have been written later?

Moreover if this ordering of the dialogues is correct, it is possible to trace a pattern of development in the occurrences of the argument. For in the *Timaeus* one of the new departures is that the World, though a unique thing, is nevertheless required to have a Form. But if the singularity of the sublunary thing does not defeat the requirement that it should have a Form, and if the structure of the arguments are as I have suggested, it is but a short step to the *Parmenides* view that the singularity of the Form does not defeat the requirement that it, too, should have a Form . . . and so on infinitely.

G. C. NERLICH

University of Leicester

NOTE ON PLATO'S *PARMENIDES* 147c

I DOUBT whether anyone would wish to take issue with Mr. Robinson's observation that there are rather "crass sophisma" in the second part of the *Parmenides*.[1] However, in his anxiety to refute Cornford, he seems to me to have chosen an unfortunate example of a sophism.[2] The argument by which Parmenides shows that the one is like the others in 147c not only is valid, but points out an interesting logical consequence which Plato could quite well have seen.[3] The argument is simply that the one is different from the others, and likewise the others from the one; thus, since they are different from each other in the same way, they are alike. The only requirement on like objects is that they have a common property (*τό ταὐτόν που πεπονθὸς ὅμοιον* 139e). Parmenides indicates a common property between the one and the others, and concludes that they are alike. The only way the argument could be conceived as fallacious is to read into it that the one and the others are the same in the same respect in which they are different. An analogous argument to such a fallacy would be that because red object R is red and blue object B is blue, they are the same; this would be a sophism. Nevertheless, just because the difference between R and B can be stated, another predicate can be stated in which they both are the same, *e.g.* each is red or blue, "*οὔτε μᾶλλον οὔτε ἧττον*". The latter argument is sound, and in my opinion closer to Plato's, although his is more indirect. If we were not told that R was red and B blue, but were merely given that R was different in colour from B, what would be their common property? Speiser has correctly argued that since 'difference' is a symmetrical relation, a common property would be that they serve as terms in the relation of difference.[4] However, the argument can be easily extended to include relations that are not symmetrical: *e.g.* for any dyadic relation of the first order, the related individuals have the common property of belonging to a couple which satisfies that relation.

The argument shows an interesting effect of defining a class as composed of members which are all alike in some way, *viz.* a common quality always can be specified by which any things whatever can be considered a class, even if we are not often inclined to think of the queen of hearts, Plato's beard, and the Eiffel Tower as a class having a common quality.

JAMES L. CELARIER

University of Pennsylvania

[1] Richard Robinson, *Plato's Earlier Dialectic* (2nd edn. Oxford, 1953), pp. 248-249.

[2] The validity of both of Mr. Robinson's examples is defended in Cornford, *Plato and Parmenides* (London, 1939), pp. 124, 165; and Andreas Speiser, *Ein Parmenideskommentar* (Leipzig, 1937), pp. 23, 35.

[3] Plato again struggled with the problem in *Philebus* 12e sqq.

[4] Speiser, *op. cit.* p. 35.

VII.—CRITICAL NOTICE

G. E. Moore: A Critical Exposition. By ALAN R. WHITE. Basil Blackwell, Oxford, 1958. Pp. 226. 25s.

MR. WHITE's book contains three chapters on Moore's "philosophical method" in which he undertakes to describe the roles of "common sense" and "ordinary language" in Moore's work, three chapters on Moore's idea of philosophy as analysis, one on Moore's ethical theories, one on his contributions to the philosophy of perception, and a final chapter, entitled "The Historical Setting", in which some attention is given to Berkeley and Reid as philosophical ancestors of Moore and the remainder devoted to connections between Moore and twentieth-century contemporaries. There is a complete bibliography of Moore's books and articles, and the text contains multitudinous references to Moore's writings.

The reports of Moore's views are sometimes in error. For example, White says that Moore held that "Any theory which has results which contradict those things which 'we certainly all do, in ordinary life, constantly believe' is *ipso facto* to be considered wrong" (p. 11). But Moore does not say this in any of the passages cited. He does say in one of them (*Some Main Problems*, p. 135) that a certain philosophical view about matter implies that what we believe when we believe that the railroad car in which we are riding has wheels is not that there really are wheels there now, when nobody sees them, but merely that we should under certain circumstances perceive the sense-data that are the appearances of the wheels—he does say that this is "an utterly false account of what we do believe in ordinary life". The theory is false, according to Moore, in so far as *it purports to be an account of what we believe*. This does not provide the slightest evidence that Moore held that even if a philosophical theory does not attempt to be an account of what everyone believes it should nevertheless be considered false because it contradicts what everyone believes. Moore did not ever hold this. White's attribution of this view to Moore is, furthermore, inconsistent with his true comment *on the same page* (p. 11) that Moore "held that some common sense statements are quite definitely false". (By a "common sense statement" White apparently means here a statement expressing a universally or at least commonly held belief.) Moore says that in some respects "the views of Common Sense have changed": for example, there was a time when everyone believed that "the heavenly bodies were small compared to the earth, and at comparatively short distances from the earth"; "we have *discovered* that they were wrong" (*Some Main Problems*, p. 3). So Moore was cognizant of the well-known fact that universally or widely held beliefs sometimes turn out to be false. He did not hold that if a philosophical theory has results which contradict some common belief it "is

ipso facto to be considered wrong". It is not the case that "Moore often said and implied that universal acceptance is a reason for holding a view to be true" (p. 12). Moore's position is more subtle than White makes out. In "A Defence of Common Sense"[1] Moore puts down a long list of propositions which he calls "truisms". He declares that he knows all of them to be true, and further declares that "very many" human beings know corresponding propositions to be true. He thinks that this position of his could be expressed by saying that "I am one of those philosophers who have held that the 'Common Sense view of the world' is, in certain fundamental features, *wholly* true". It is worth noting what he goes on to say: "The phrases 'Common Sense view of the world' or 'Common Sense beliefs' (as used by philosophers) are, of course, extraordinarily vague; and, for all I know, there may be many propositions which may be properly called features in 'the Common Sense view of the world' or 'Common Sense beliefs', which are not true, and which deserve to be mentioned with the contempt with which some philosophers speak of 'Common Sense beliefs'. But to speak with contempt of those 'Common Sense beliefs' which I have mentioned is quite certainly the height of absurdity" (*op. cit.* p. 207). Clearly there is no warrant here for saddling Moore with White's sweeping principle. White does not observe Moore's cautiousness.

There are other examples of a careless reading of Moore. White quotes a former remark of mine, which he calls "the argument from the standard example or paradigm case": "In the case of all expressions the meanings of which must be *shown* . . . it follows from the fact that they are ordinary expressions in the language, that there have been *many* situations of the kind which they describe; otherwise so many people could not have learned the correct use of those expressions." White makes the comment that this assertion "seems parallel" to Moore's contention that many of the "truisms" that he listed in the "Defence" have the "peculiar property" that "*if they are features in the Common Sense view of the world (whether we know this or not), it follows that they are true*, since to say that there is a 'Common Sense view of the world', is to say that they are true" (p. 36). I do not see any parallel or even any connection. What Moore meant here is not clear to me. I *think* he meant that the proposition "There is a Common Sense view of the world" entails a number of propositions. Among them are "Time is real", "Space is real", "Many human beings who have had bodies have lived upon the earth". *If* there is a Common Sense view of the world then the entailed propositions are true. According to Moore those propositions are themselves features of the Common Sense view of the world. The proposition that those propositions are features of the Common Sense view of the world entails the proposition that there *is* a Common Sense view of the world. This

[1] *Contemporary British Philosophy*, 2nd ser., ed. Muirhead, Macmillan, 1925.

latter proposition, as said, entails those propositions. Thus *if* the latter are features in the Common Sense view of the world it follows that they are true. Whether or not this is a correct interpretation of Moore's thought, there is no evidence whatever that Moore was making here an "argument from the standard example or paradigm case".

White's book is "a critical exposition", but the criticism is never impressive and is often unclear. For example, Moore makes the interesting claim that the proposition "The sentence 'At least one person is a King of France' means that at least one person is a King of France" is *not* a tautology. He has two formidable arguments: (1) that the above proposition is *the same proposition* as "*Les mots* 'At least one person is a King of France' *veulent dire qu'une personne au moins est un roi de France*", and that this latter is obviously not a tautology; (2) that the words "At least one person is a King of France" *might not* have meant what they do mean. Calling those words "Z", he says: "Z *might*, quite easily, *not* have meant that at least one person is a King of France. To say that it does mean this is to say something about the correct English use of the words which occur in Z and of the syntax of Z. But it might easily not have been the case that those words and that syntax ever were used in that way: that they are so used is merely an empirical fact, which might not have been the case. There is, therefore, no *contradiction* in the supposition that Z does *not* mean that at least one person is a King of France: it *might* have been the case that it did not."[1] White says against the first argument that "Moore has confused the question whether a statement is a tautology with the question whether someone knows that it is a tautology" (p. 103), but his explanation of this is so brief and obscure that I have not been able to puzzle out his meaning. Against Moore's second argument he says that "the fact that an expression might have meant something other than it does mean does not lead to the conclusion that the statement that it does mean so and so is a non-tautological statement. Moore thinks that it does because he moves from the correct view that a tautology is a statement which remains true however the world may change to the incorrect view that a tautology is a statement which remains true even if the meanings of our words change. Tautologies are not, as he seems to think, statements which cannot be false, but statement which, provided we mean what we do mean, cannot be false" (pp. 103-104). He says that Moore "does not realize that the notions of self-contradiction and tautology can only be used in relation to a given language. To say that so and so expresses a tautology is elliptical for saying it expresses a tautology in such and such a language" (pp. 105-106). It is hard to make any sense out of this facile criticism, or to see what its application is to Moore's argument. "Tautologies are not . . . statements which cannot

[1] "Russell's 'Theory of Descriptions'", *The Philosophy of Bertrand Russell*, ed. Schilpp (Northwestern, 1944), pp. 202-203.

be false, but statements which, provided we mean what we do mean, cannot be false". Is White proposing an argument like the following: If "Z" means that Z, in language L, then "'Z' means that Z' is a tautology in L; "Z" does mean that Z in L; therefore "'Z' means that Z" is a tautology in L? This argument, if it is White's is incoherent: the second premise, "'Z' means that Z, in L", is presumably either a tautology or not a tautology; if White regards it as a tautology then his argument begs the question: if he regards it as not a tautology his argument is self-contradictory. This is my best guess as to how White undertakes to prove, contrary to Moore, that "'Z' means that Z" is a tautology. But White's text does leave one guessing. He has not been infected by Moore's pure style and immaculate reasoning.

I imagine that White thinks his major contribution to studies of Moore lies in his treatment of the relationship between "common sense" and "ordinary language" in Moore's philosophy. It occupies the most prominent position in his discussion of Moore's method to which, he says, he gives "pride of place". He maintains a thesis, namely, that some previous commentators on Moore, chiefly myself, have "confused" or "assimilated" Moore's "appeal" to ordinary language with his "appeal" to common sense. This, he says, is an "enormous howler" (p. 5). Malcolm sometimes confuses the "two appeals", but more often "he assimilates them while admitting that they are distinct; and, therefore, tries to suggest that while Moore thought he was appealing to common sense he was really appealing to ordinary language; and that while the latter appeal is legitimate, the former is a mistake" (p. 6). The true fact of the matter, says White, is that "Moore's recourse to ordinary language . . . is mainly intended to discover what a philosopher's view comes to when put into the language we all understand, namely, ordinary language, and to indicate what in fact are the beliefs of common sense by referring to what we all ordinarily say. Having established what is the philosophical view and what the common sense view and that the two conflict, he can then use the appeal to the latter to refute the former. The appeal to ordinary language is, for him, mainly subsidiary to the appeal to common sense. Malcolm and those who agree with him have reversed the correct relative position of the two" (p. 7).

In my own writings on Moore I have not devoted any particular attention to his practice of setting out the implications of a philosophical position in terms of concrete examples expressed in ordinary language: *e.g.* of drawing from the philosophical proposition "All that we know of material objects is the orderly succession of our own sensations" the alleged consequence that a person riding in a train cannot know, at the time, that the train has wheels. Moore was uncommonly skillful at this. I should have thought that the merit of *this* "appeal to ordinary language", if White wants to call it that, was beyond dispute and I agree that it is an entirely different

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thing from Moore's defence of Common Sense. When I conceived of Moore's defence of Common Sense as a defence of ordinary language I did not mean by the latter the above practice. What I did mean can be understood only in terms of the theory that I formed about his defence of Common Sense. I will briefly restate it because it still seems to me to be sound.

Prima facie common sense has nothing to do with philosophy. How do we actually use the expression " common sense "? Someone says : " If you want so very much to enter the Civil Service then it is common sense that you must begin to prepare youself for the examination." " It is common sense " means here : it is the obvious practical conclusion. Something falls under the heading of " common sense " if it is an obvious conclusion from information at hand. The information would be of a sort that was useful in daily life and the conclusion would be a practical one. In this common use of " common sense " it has nothing to do with views about the universe nor with what Moore calls the " belief " that " there are enormous numbers of material objects in the world ". " He is lacking in common sense " means : He is lacking in good judgment. As Thomas Reid says : " A man of sense is a man of judgment. Good sense is good judgment. Nonsense is what is evidently contrary to right judgment. Common sense is that degree of judgment which is common to men with whom we can converse and transact business." [1] In the common uses of the phrase " common sense " it is not the title of some set of beliefs. Shall we say then that Moore used the expression " common sense " in an uncommon sense to refer to some set of common beliefs? Here too we run into a difficulty. The things that he lists as universal or wide-spread beliefs are a queer assortment. Included are such things as " There are enormous numbers of material objects ", " Men, besides having bodies, also have minds ", " The sun is many times larger than the earth ". The first two are not *beliefs* at all. The third is genuinely a common belief or, indeed, common knowledge : but what is the relevance of it to philosophy? : what philosopher has denied it? and if one did would this be a philosophical view? In order to see Moore's defence of Common Sense at work, in the aspect of it that makes it an important contribution to philosophy, one must consider such a passage as the following : " If Hume's principles are true, then, I have admitted, I do *not* know *now* that this pencil—the material object—exists. If, therefore, I am to prove that I *do* know that this pencil exists, I must prove, somehow, that Hume's principles, one or both of them, are *not* true. In what sort of way, by what sort of argument, can I prove this?

" It seems to me that, in fact, there really is no stronger and better argument than the following. I *do* know that this pencil exists ; I could not know this, if Hume's principles were true ; *therefore*

[1] *Reid's Essays on the Intellectual Powers of Man*, ed. Woozley (Macmillan, 1941), pp. 330-331.

Hume's principles, one or both of them, are false. I think this argument is as strong and good a one as any that could be used: and I think it really is conclusive" (*Some Main Problems*, pp. 119-120).

Now what can be the value of this apparently question-begging rebuttal of Hume? The theory I put forward and still hold to is the following. When a philosopher maintains that we do not know material object propositions to be true (Hume) or that we do not see material things (Prichard), he must be holding that the notions of knowing a material object proposition to be true and of seeing a material thing are really *self-contradictory*. He certainly cannot be maintaining that it is a matter of experience that people do not know or see such things. He must be trying to state what he thinks is an *a priori* truth. He must be claiming, perhaps without fully realising it, that ordinary sentences like "I know that that thing sticking up in the garden is a shovel", "I see your glasses under the bed", are self-contradictory. To understand that this claim is mistaken it is sufficient to realize that these sentences do have a correct use in ordinary discourse, which they could not have if they were self-contradictory. The important function of Moore's rebuttal was simply to remind us of this fact. When Moore declared "I am not . . . afraid to say that I do now perceive that this is a door, and that that is a finger" (*Philosophical Studies*, pp. 226-227) it did not matter whether he was actually looking at a door or a finger. He did not have to produce an example of a true perceptual statement. He did not have to present a *paradigm* of seeing a material thing.[1] All he needed to do was to remind his audience and readers that the sentence "I see a door over there" can be correctly used to make a true statement. As Moore himself says, "It seems to me that we do, in ordinary life, constantly talk of *seeing* such things, and that, when we do so, we are neither using language incorrectly, nor making any mistake about the facts—supposing something to occur which never does in fact occur" (*Phil. Studies*, p. 226). He was not begging the question against Hume or Prichard because his very point was that it is really not open to question that such sentences have a correct use. On my view then, Moore's so-called defence of Common Sense, in so far as it is an interesting and tenable philosophical position, has nothing to do either with common sense, properly speaking, nor common belief, but is merely the assertion, in regard to various sentences, that those sentences have a correct use in ordinary language. This makes it into a very simple idea, but that is what good ideas sometimes are. It is in this way that I characterized Moore's "defence of Common Sense" as being in reality a defence of ordinary language. I should like to learn where the howler is in this interpretation.

[1] I failed to see this clearly when I first wrote on Moore. See *The Philodophy of G. E. Moore*, p. 354: "What his reply does is to give us a *paradigm*. . . ."

Except for parts of the chapters on philosophical analysis White's book is regrettably dull. The reports and classifications of Moore's opinions and arguments are tedious. In the final chapter, the comparison of Moore to Reid and Berkeley is too brief to be of any value and the second half, on Moore's contemporaries, is a *pot-pourri* of rapid and superficial remarks about a host of philosophers. It is difficult to imagine what the author thought the good of it might have been. Lacking depth, accuracy and fresh insight, this work will hardly remind the reader of the exciting qualities of Moore's writings.

Cornell University NORMAN MALCOLM

VIII.—NEW BOOKS

The Logic of Scientific Discovery. By KARL R. POPPER. Hutchinson, London, 1959. 50s.

THE contents of this untidily composite volume are as follows. There is first a translation of *Logik der Forschung*, made by the author with the assistance of Dr. Julius Freed and Lan Freed. This is greatly to be welcomed, for that influential book has been, in the twenty-five years since its publication in Vienna, often misrepresented and too seldom read. Here to the text and notes as first issued in 1934 is added a number of new footnotes, but the original text has been left unmodified. Next comes a translation of seven appendices that were printed with the original text, to which again has been added a number of new footnotes. Lastly there are twelve new appendices, covering 150 pages, some of which are notes and articles re-printed from various periodicals. Throughout the book there occur, besides copious references to earlier writings by the author, allusions also to his future publications; there is to appear another volume dealing with the same range of problems, to be entitled *Postscript: After Twenty Years*. The outcome of this process of accretion would have been more confusing than it is but for the fact that the author—to his surprise, as he says—has not come to disagree in any major respect with the views that he propounded in *Logik der Forschung*. That being so, the additions involve amplification and development—as well as, unfortunately, repetition—rather than alteration.

The main topic of the book, as no doubt is sufficiently well known, is the analysis of scientific theories and scientific method. Somewhat aside from this, though developed at length particularly in the new appendices, are discussions of the theory of probability, and of some of the notoriously controversial features of quantum theory in physics. In these latter fields the author's technical equipment is imposing, and the present reviewer would find it difficult, as well as imprudent, to comment on his performance. On the celebrated issue of 'falsifiability', however, there are brief comments to be made, and even some doubts to be aired.

As will already be known to many, the most striking and original feature of Popper's study of the logic of science is his extreme hostility to what he calls 'inductivism'; he stands, as he puts it, "directly opposed to all attempts to operate with the ideas of inductive logic". Now in this programme of rejection there seem to be at least three distinguishable elements which, though it would be unfair to say that they are confused by Popper, are not always clearly distinguished by him. But it seems important to distinguish these elements carefully, for while two are meritorious, one is not.

The first thesis which Popper includes in his rejection of 'inductivism' could perhaps be expressed as follows: inductivism misrepresents the procedures of scientists. That is, scientific theories are not arrived at by the massive accumulation of observations leading, by induction, to a generalization; nor are scientific experiments to be regarded as observations to be added, if favourable, to any such accumulation. It is, on the contrary, far more realistic to say that theory precedes observation. To guess, to venture a hypothesis, is the essential first step: one may then deduce what should be found to occur if the hypothesis is tenable; and

experiments may be made in order to find out whether that does occur. Thus the essential function of experiments is to test hypotheses already put forward, and not, as inductivism implies, in some way to generate hypotheses from a ruck of initially undirected observations. The thesis thus baldly outlined will be, and indeed already has been, widely accepted.

Secondly, I believe that Popper also intends to deny that there is any such thing as the logic of induction. Some at any rate of his strictures on 'inductive logic' seem to have this sense. It seems likely that he would wish to say on this point not only that it is impossible to lay down formal rules for assessing the validity of inductive arguments, but that, more sweepingly, inductive argument as traditionally conceived does not merit the name of 'argument' at all. Here also the conclusion, if not the more radical ground for it, would be widely accepted.

We come now to the third element in Popper's rejection of 'inductivism', and it is here that, I think, he can be seen to be claiming too much. He points out, quite rightly, that accounts of scientific procedure which represent the scientist as seeking to arrive at general truths by induction from observations all raise, apart from serious objections on the score of unrealism, what he calls 'Hume's problem', or simply 'the problem of induction': what general warrant can there be for extrapolation from known or observed to unknown or unobserved cases? Now Popper says emphatically that this venerable problem is insoluble, since any warrant in the form of a 'principle of induction' must either be *a priori* and therefore ineffective, or synthetic and therefore productive of an infinite regress. Accordingly he wishes to claim for his own views, not that they offer a solution of Hume's problem, but that they do not permit it to arise. However, I think it can be seen that, if there is such a problem, it is raised no less by Popper's views than by anyone else's.

His argument runs thus. It has been usual to ask how, on the basis of a finite corpus of particular observations or experiments, we can be justified in regarding as true, or even probably true, the general statements of, for instance, a scientific theory. There is, Popper holds, no hope of answering this question, since it requires that we should solve the insoluble problem of induction. But, he says, it is needless and misguided to ask this question at all. The question to address to a scientific theory is not whether it is true, but simply whether it stands up to tests; if it does, it is, not verified, but (so far) corroborated—it has "proved its mettle". Since we are not to ask of a theory whether it is true, there is here no question of arguing from singular truths to general truths, so that the problem of our justification for so doing does not arise. If observation confirms those singular statements which are entailed by a scientific theory, then the theory is tested and not falsified; and to be thus not falsified, in spite of rigorous testing, is to be corroborated—the theory thus "proves its fitness to survive". We do not, indeed, show it to be true; but that is not to be lamented, for in principle it could not be done. However, it is certainly possible for experiment to show a theory to be false; and a good theory is one which has defied our best efforts to do this.

Now does this eliminate the "insoluble" problem of induction? Surely not—it moves that problem to a different point. This could be shown in several ways. How, for instance, by the fact that it *has* withstood rigorous tests, does a theory, in Popper's phrase, "prove its fitness to survive"? It does so, clearly, on the supposition that what has withstood tests also will withstand them in future. Even if we do not venture upon the inductive inference that a theory will, or would, be not falsified by other

tests than those made hitherto, we must suppose at least that it would again withstand the very same tests, if they were repeated. Indeed, Popper rightly stresses that a significant testing procedure must be repeatable, and this would be pointless without the supposition that the same test will, if repeated, yield the same result. What warrant then, as Hume would ask, do we have for making this supposition? Why should we suppose that like causes will have like effects? Alternatively, if, as Popper sometimes says (*e.g.* pp. 42 and 265), it is to be analytic that a theory which has withstood rigorous tests is "well corroborated", it may be asked what connection there is between a theory's being well corroborated, and its being properly to be accepted and relied upon in future cases. Popper evidently assumes, what of course his language implies, that we are entitled to rely upon a well-corroborated theory. But how are we so entitled, if all that "well-corroborated" means is that the theory *has* withstood our efforts to falsify it? What has this fact about its past to do with its future prospects? This may, indeed, be a foolish question; but it is, again, Hume's question; and if Popper holds, as he seems to, that it is unanswerable, then his own views clearly lead him up to an unanswerable question. If, on the other hand, he should try to reject the question as improper or misconceived, then he ought to have performed the same service for those 'inductivists' whom, elsewhere, he has represented as the helpless victims of Hume's dialectic.

This is not to say, of course, that Popper's account of the status and character of scientific theories is not, in many respects, a vast improvement upon those accounts which he calls inductivistic. It is certainly much more realistic than most, and it is subtly and skilfully worked out in detail. The present point, which may however be of some philosophical interest, is only that, if there is such a thing as the problem of induction, it is a problem for Popper as much as for anyone else.

One point from the discussion of probability should be mentioned. Some readers may be puzzled by Popper's repeated insistence that a high degree of corroboration in no way increases the probability of a hypothesis, and that indeed the probability of a "universal law" always remains, notwithstanding all tests and confirmation, zero. I believe—though I may well be mistaken here, and certainly do not understand why Popper claims so much importance for his point—that this is a needlessly paradoxical way of saying that acceptability of a scientific statement is not to be identified with probability, in the sense in which the calculus of chances treats of probability: and that, in that sense, the chance that a particular universal law should have held is, given the innumerable other possibilities, practically zero, even though we may have good reason to think that it does hold in fact. Popper is not then really saying, though he frequently seems to be, that it grows less and less likely that a statement is true as we accumulate more and more evidence in its support.

G. J. WARNOCK

The Reach of Science. By HENRYK MEHLBERG. University of Toronto Press, 1958. Pp. xii + 356. 45s. net.

SCIENCE is beginning to be a "prestige" subject. It is not surprising therefore that we should meet with attempts to show that its aims and methods have a peculiar power to solve all our problems for us. Professor

Mehlberg, in this book, is the latest to demand acknowledgment of the "universality of science". His version is that "if a problem is solvable at all, it can be solved, in principle, by applying scientific method" (p. vii). What of those problems, that are, on the surface at least, "non-scientific" as such; for instance, policy-making decisions, aesthetic evaluations, or moral puzzles? It should not be thought that the author has lost sight of this issue, for it is one of the main purposes of the book to pursue its various ramifications. Clearly, it will be necessary to make the definition of "scientific method" wide enough to accommodate a large number of fish; at the same time, value problems will have to be distinguished in such a manner as not to invalidate the thesis in an obvious way.

As to the first point: The required generalisation of the concept of scientific method is facilitated by the view that verification and proof rather than the "special methods" of science (p. 73) or the procedures of discovery form "the gist of scientific method" (p. 37). And so it turns out that "applying scientific method" is tantamount to seeing whether a proposition is verifiable in the light of observational evidence (*ibid.*). This concept of verifiability is hence the keystone of the whole edifice because (in the end) we are to understand by scientific method the principle that a proposition is *either* verifiable *or* it is neither true nor false (pp. 40, 331). But not meaningless! For the traditional verifiability theory of meaning seems to the author "utterly untenable" (p. 41), partly because a place has to be found for the class of unverifiable statements that play such a large part in developed scientific systems (pp. 301, 307). So now we see on what the claim to the universal "reach of science" is based.

The verifiability principle itself, instead of being a "mere postulate", is claimed to be "deduced from definitions of truth and verifiability" (p. viii). "Anyone who correctly understands the adjectives 'true' and 'false' cannot help realising" the adequacy of the principle (p. 44). A considerable part of the book is hence devoted towards an elucidation of the concept of truth. Despite one disclaimer (p. 277) it seems to me at least that truth is considered tantamount to "correct linguistic behaviour" (pp. 271, 274-275, 333). A "true ostensive sentence" is defined as one "whose subject and predicate both denote the same individual" (p. 277) where "an ostensive predicate denotes a particular individual, if everybody who perceives this individual and who uses this predicate correctly would apply it to this individual" (p. 333). Where you cannot do this, *e.g.* where the referent is vague (pp. 322-331), *i.e.* where you cannot verify, you *ipso facto* don't have truth (or falsehood) (p. 331). I cannot see that this is any more successful than previous similar attempts to define truth in conventionalist or behaviourist terms. It seems to me simply obvious that I may behave linguistically correctly and yet be dead wrong! This conclusion is concealed no doubt by the fact that "correctness" may of course be used so as to be *synonymous* with true.

The second problem, of how to get scientific method to cope with value judgments, is dealt with rather more summarily (*cf.* pp. 34-35, 337-338). The position here adopted vacillates considerably; sometimes there is a suggestion that value statements are (or should be) completely verifiable; at others, it appears that what is verifiable is a statement which is a "logical consequence of observational premisses involving the relevant value-predicates" (p. 338). The context of the discussion suggests that this amounts to saying that value statements are verifiable in the sense in which one might say that Picasso is preferable to Annigoni because Picasso discovered that instead of copying human shapes one may convert them into

geometrical designs; and then show that *this* is true because he in fact does so convert them!

Professor Mehlberg in general claims "non-partisanship" (p. 270) concerning a number of philosophical issues (*e.g.* nominalism and phenomenalism *versus* realism) but the result is that the argument at times becomes somewhat laddered and anaemic. On the other hand, these remarks which turn on the main thesis of the book fail to do justice to the very thorough way in which the argument has been supported by a very systematic survey of the more conventional themes of traditional "philosophy of science". This, rather than the issue of truth may eventually turn out to make this book a very useful one indeed for instructors in the subject. Thus one third of the work consists of a careful if traditional treatment of such topics as fact-finding methods in science; types, functions and validation of scientific laws; methods of theory formation. Elsewhere there is a discussion of status questions, *e.g.* the status of mathematics, including a very brief discussion of the axiomatisation of arithmetic (pp. 51-59) which somehow fails to mention the place of associative and similar laws; the problem of the status of theoretical entities which is discussed under various headings (pp. 85-88, 145 ff., 285-292, 267-269) and the whole question of the logical relations holding between basic and derivative propositions.

Here and there one senses a curious impersonal opaqueness about difficulties which the author seemed elsewhere to have faced plainly. Thus, in connection with the problem of unobservables (after previous quite careful discussion) we are suddenly told that "loosely speaking" they "may be said to reflect themselves in the world" of observables (p. 269). This is explained as meaning that expressions containing non-observables can be "deduced" from true statements concerning observables. Why "*deduce*"? This connects with a curious attempt to try and avoid where possible the terminology of induction or even inverse-deduction. Thus a universal sentence is defined as being the "logical consequence" of the class of its instances even where that is infinite (p. 272). Elsewhere, we are told that such statements are "inductively verifiable" if *every* instance of them is directly or finitely verifiable (p. 298). This must not be taken to mean that the author has not perceived the puzzle of inductive inference, which receives some discussion elsewhere (pp. 195-199, 229-231) and to which he professes to have no final answer. Corresponding to the concept of inductive verifiability of laws (as just given) we also get one for theories (p. 230). Here the expression "the theory T" is replaced by the "universal law" to the effect that *every* verifiable consequence of T is true. I have italicised "every" in each case so as to emphasise how this account involves a sophisticated procedural definition of verifiability which may trick one unawares into a logical blunder. For, how much does "every" include? Is *this* not the puzzle of induction? A qualm which is not assuaged when we read that "the probability that a single new verifiable consequence of the theory will prove correct is provided by a representative sample of such consequences" (p. 230). But no guidance is provided for making a sample a representative one!

Space forbids mention of many of the topics discussed, *e.g.* the nature of measurement; definition theory; etc., each stimulating and instructive. One fault that one may find with the book is perhaps that although superficially it appears to be well-constructed, closer reading reveals that one has to search for a full discussion of certain central topics in a large number of varied places. Moreover, there is a certain technical

detachment, occasionally amounting to jargon, which at first sight may seem somewhat repellent particularly to the English reader. Nonetheless, this is obviously a work which (though uneven) will find a place in the hands of the student of the traditional fields covered in the philosophy of science as well as those of the general philosopher.

GERD BUCHDAHL

Retreat from Truth. By G. R. G. MURE. Pp. viii + 255. Basil Blackwell, Oxford.

THIS book is, for two parts of its length, an eruption of wrath, sometimes splenetic and petulant, sometimes reasoned and splendid, against philosophical traditions of which the author disapproves, and which have been, and are, the dominant traditions of our country, and, for its remaining third, a fine, if somewhat fuliginous, statement of the sort of philosophy of which he approves, and which is now suffering a reverse of fashion. It abounds in rhetoric of a kind often found in Plato, but perhaps more characteristic of a work like Carlyle's *Past and Present*, with which it challenges comparison. It is to be feared that it will largely fail of its purpose, since those whom it is designed to unsettle will be so irritated by its attacks on their new-found security and valued expertise, as to be unwilling to penetrate the obscurities of the doctrine with which it ends. This will be a pity, since Mure has much to teach the contemporary philosopher, which the latter is, in fact, not very far from finding out for himself, and since his work, though immoderate in tone and historically tendentious, seems a model of impartiality and historical *approfondissement* when set beside certain recent reconstructions of the past.

In the first part of his book Mure characterizes the philosophy of the age we live in and relates it to the cultural background of the times. Modern British philosophy, on his view, is utterly reactionary in character: it has harked back to an unsubtle empiricist and mathematico-scientific tradition after a brief period of apprenticeship in, and practice of, the much subtler, flexible thought that it learnt from the Germans and the Greeks. And as our brief experience of synoptic thinking was a product of our unparalleled, late nineteenth century material prosperity, so our reversion to type is a product of the economic stresses and hot and cold warfare of the present age. Mure does not explain why our German cousins were positivists in the heyday of their Bismarckian prosperity and existentialists in their precarious present, nor why our American cousins, whose curve of prosperity has not declined at all, should have had a similar philosophical history to our own. Mure holds that our empiricism, as well as the mathematicized natural science that completes it, is an expression of an attitude which, following Croce, he calls 'economic': it is the approach of the self-interested individual concerned to subordinate the world *practically* rather than to plumb its profundities. This 'economic' approach is then traced through the thought of Locke, Hume, Adam Smith, Bentham and the American pragmatists.

It is not easy to know how to meet all these sweeping assertions with anything but dissent. I should myself agree that there is a *purblindness* about our Anglo-Saxon intelligence which makes us bad philosophers in certain directions. We are much too keen on the hard datum and the reliable connection, a trait which explains both our empiricism and our logic-chopping, in which our pre-eminence goes back to the time of Ockham

and Roger Bacon. And quite possibly this extreme caution *has* a banausic origin: we shun doubtful inferences like doubtful investments, and will in the last resort risk asserting nothing beyond what is 'logically entailed' by some proposition or notion, *i.e.* nothing beyond the actual content of that notion or proposition itself. And it is a caution which has made us largely insensitive to the fine web of non-rigorous implications which forms the interstitial tissue of every kind of discourse, and without which all really will resolve itself into 'fractured atoms', just as Mure contends. Can it, however, be maintained that we have not the qualities of our weaknesses? Does not the pursuit of non-rigorous implication often end in logically undisciplined riots? And does not the philosophy of the hard datum and the firm connection represent at least an indispensable and permanently valuable side of philosophical thought? Hegel, at least, would have held it so, and would have ascribed to such thought no banausic origin. It may in fact be held that a philosophy of the Understanding, in Hegel's sense, is our genuine and characteristic *métier*, and that we have managed even in this period of decline, to produce one who is perhaps the greatest of all philosophers of the Understanding, *i.e.* G. E. Moore. Beside this immense, almost Socratic figure it seems absurd to say that we must borrow all our good philosophizing from the Germans or the Greeks.

In the second part of his book Mure makes a careful study of Russell's *Analysis of Mind* as a typical instance of the economic empiricism that he wishes to discredit. The instance is not particularly well chosen. The *Analysis of Mind* is a work with a brilliant *idée mère* which has been executed with scandalous shoddiness. Only the wit and style of the author makes it more than it officially was: a set of semi-popular lectures given to an audience in Peking. The Warden of Merton gravely exposes Russell's various makeshift improvisations, *e.g.* belief-feelings, with which he wards off his successive difficulties: he would have done better, in quest of a sound product of British Understanding, to have dissected a work like Broad's *Mind and its Place in Nature*, in which incidentally Russell's *Analysis* is playfully laughed at. Mure has several pertinent criticisms to make of the servitude to symbolic logic in which analytic philosophy was reared and from which it has only now been well weaned: he then gives an interesting outsider's account of the march of thought from the *Tractatus* through the Vienna School and Ryle, down to the Wittgenstein of the *Investigations*. The last is described as a good one-eyed philosopher among the contemporary blind, and as one who, though unhappy in his philosophical education and limited in his thought-horizons, none the less managed to break the tetanus into which the *raideur britannique* naturally tended to develop, and led it back to more fluid, continuous, almost Bosanquettian ways of thinking. He remained, however, a positivist, and his pluralism of language games preserved the atomism that it sought to dissolve. This estimate of Wittgenstein is curious, but perhaps not so far from what we shall ultimately think of him as many would like to suppose. For it seems clear that his original policy of philosophical dissolution was employed upon the quite narrow range of Cambridge ideas in which he was nurtured, that he destroyed them with their own techniques, and that his activity largely represents the *self*-dissolution of a short-lived, if brilliant, philosophical movement.

The third part of Mure's book is somewhat obscurely called 'Value'. Of Value Mure says that it eludes 'economic observation', that it is a 'toti-resultant' property of a whole in which certain characters can be

'economically' discriminated, that it emerges when the abstraction which leads to the emergence of such characters is relaxed, and is not a new character superadded from outside. He says, too, that Value is inseparable from the mind's self-conscious activity, that it exists only in so far as there is an endeavour to transcend the singularity of the individual person, and in so far as there is an endeavour to transcend the singularity of *all* actual persons or of the actual systems which they form. Mind, in short, is credited with the impersonal universality and 'infinity' attributed to it by Hegel, and it is with these traits that 'Value' is connected. Mure shows how this *nisus* towards unlimited self-transcendence is involved in all the things and experiences that we call good. It is present in the systematic ordering of experience by science, even though the scientist himself rarely sees it, being more intent on the fragmented abstractions that lie in front of him, than the illumination they cast behind them. 'If you look back from science to the concrete world it abstracts from you will see the value on which science turns its back.' It is also present in moral action, where 'the will of the singular individual is actively identified with a will which is beyond him as well as within him'. Aesthetic vision, too, is 'never the enjoyment of a private and particular good': in it an unclear personal emotion is embodied in the world, so that it becomes clear and shareable by all. Philosophy, finally, is for Mure a study which uses the notion of value or self-transcendence to explain objects and conscious activities, and which must immerse itself in its own past history which it has transcended in its present phase. In this connection Mure excoriates Oxford and its contemporary teachers who have tried to treat the study of past philosophy as a mere piece of 'philosophical palaeontology'.

It cannot be claimed that the message of Mure's last section is well expounded, or that it will convert those whom he would wish it to convert. It rests, none the less, on Kantian and Hegelian conceptions of the utmost depth and interest. It rests on a view of mind for which it is in no sense accidental, nor a mere matter of empirical fact, that we stand confronted with materials systematically describable in terms of fairly restricted linked sets of universals, and so qualifying as 'objects', nor that we stand side by side with other intelligences, to whose witness we can appeal, and with whom we can build up a common objective picture. This last point became clear even to Wittgenstein when, with characteristic exaggeration, he was led to deny the possibility of a private language. Mure's view rests, further, on the assumption that the self-transcendence which yields 'truth' in science does not differ fundamentally from the self-transcendence which yields well-founded ethical and aesthetic judgements, and that in every sphere, therefore, considerations of what is fit, good, right, etc., are supreme and decisive. All these views are much more respectable than contemporary thought likes to think them, and its paralysis is their strength. As to Oxford, it may be doubted whether its present state differs much from any in which it has previously been. Oxford has always had its own curious map of knowledge in which the local and contemporary occupied much of the *orbis terrarum*, beyond which lay mainly the anthropophagi or pre-history. If its present ignoring of much synoptic thought is to be regretted, how much more to be regretted was its past ignoring of whole disciplines and territories of knowledge, and its epigrammatic dismissal of considerable thinkers? One can only hope that, under its protective rampart of beauty and ignorance,—sometimes very *docta*—it may continue to make noteworthy contributions to philosophy.

J. N. FINDLAY

The Later Works of Wittgenstein. By DAVID POLE. Athlone Press, 1958. 15s.

DR. POLE'S lack of sympathy with Wittgenstein's ideas results in an exposition of them which is distorting, and in criticisms which display a failure to grasp their more important implications.

The following criticisms occur in chapter 2, which is on language. Wittgenstein says: "Ordinary expressions are in order as they are." This commits him to the view that, either no new ways of speaking may be introduced, since according to existing rules they would be incorrect or meaningless, or else such new ways must be said to constitute new language games. The introduction of a new language game can never be right or wrong. But extensions of existing language games do occur, and the introduction of new games is often right or wrong. So it is not true that: "Between the practitioners of two such ultimately different sets of linguistic practices no argument is possible." (*Cf.* pp. 57, 82.) The real reason for this is that there is a pattern in the world which language ought to fit (pp. 82, 83, 85). However, apart from this, Wittgenstein is involved in inconsistency, for he withholds the words "Right" and "Wrong" where ordinary usage applies them, although ordinary usage is his standard of correctness.

Now, this argument embodies some fairly central misconceptions. Wittgenstein says early in the *Philosophical Investigations* that: "The multiplicity of language games is not fixed." What is to count as one language game is not laid down by Nature, God, or Man. Nor is it laid down by the inventor of the term: he begins by listing about five possible ways of drawing the boundaries, and from then on uses the expression more or less *ad hoc*. What is to count as a new language game, as against an extension of an old one, is not only not predetermined by the nature of the world, it is only in a very few cases that it is in any obvious way predetermined by the nature of the game being extended.

One of the consequences of this multiplicity is that generalizations about ultimate linguistic practices have small chance of being true. Again, the possibility of extensions, and of new games, is clearly not incompatible with the inapplicability of "Right" and "Wrong", but is actually connected to it. The dilemma concerning ordinary usage is not genuine, for either Wittgenstein's arguments (*e.g.* those about different kinds of criteria) are the kind of arguments which can prove that ordinary usage disallows "Right" and "Wrong" as descriptions of certain states of affairs; or else the notion of ordinary usage has here become quite corrupted, and we might as well say that: "According to ordinary usage, a poet is a man frightened of honest work."

That a ban on changing language is quite foreign to Wittgenstein's ideas can be seen from the fact that such a ban is subject to the same kind of objections as those levelled against logical atomism. Dr. Pole's failure to see this is his failure to see what the attack on logical atomism was about, and is thus symtomatic of his misunderstanding of Wittgenstein's main contributions to philosophy.

JENNY TEICHMANN

Benedict de Spinoza. By H. F. HALLETT. London: The Athlone Press. Pp. xvi + 171. 25s.

PROFESSOR HALLETT has devoted many years to the study of Spinoza and the tone of his writing on this subject is devotional. He believes that in

this country and at this time, when empiricist assumptions are largely unquestioned, Spinoza has been completely misinterpreted. He does not himself share these assumptions and believes himself to be a faithful Spinozist. There is a certain obsessional quality about the relation of interpreter to master, which was apparent in *Aeternitas*, Professor Hallett's earlier book, and it is still more apparent here. Spinoza is angrily defended against every possible criticism. The calm and detachment of Spinoza himself is not to be found here.

But it is certainly useful to see every assumption of contemporary philosophy questioned in the interpretation of Spinoza. Joachim's Spinoza was still half an idealist, starting from the coherence theory of truth. The Spinoza of later interpreters is mainly concerned with the possibility of complete scientific explanation and with the moral implications of this possibility. Wolfson's Spinoza was still developing to their logical conclusion the metaphysical distinctions recognised by Philo and the schoolmen. The Spinoza of almost every French commentator (Chartier was an exception) was simply repairing the inconsistencies in Descartes. This method of approximation is rather disheartening. One turns to Professor Hallett with the hope of finding absolute impartiality in interpretation. The hope is not altogether disappointed. He does correct some widespread misconceptions and he does emphasise points in Spinoza that have recently been neglected. But Professor Hallett's style is a formidable barrier standing between the reader and Spinoza's *Ethics*: it is almost as if Spinoza had been translated into a third language, neither English nor Latin. Sentences like the following cannot be counted as exposition—"Instead of saying, with Descartes, that the thing ideated is 'contained objectively' in the idea of the thing, less ambiguously I shall say that as its epistemic terminus the ideatum modalizes the idea or mental action". It is possible to know what this means in its context. But it is not clear, and it is not the language of Spinoza. One has to disentangle the truths conveyed in Professor Hallett's terminology.

Professor Hallett rightly takes the distinction between that which is fully active and that which is relatively passive as the centre of Spinoza's philosophy, both as a theory of levels of being and as a theory of levels of knowledge. This distinction cannot be formulated without admitting a notion of the real potentialities or powers of things, and particularly of their potentialities or powers of self-maintenance and self-assertion. The individuality of a thing, the possibility of identifying it as something that is essentially unitary and separate, depends upon its activities being truly regarded as the expression of its unique powers. Thought is the activity that is characteristic of persons, and each man's thought is the characteristic activity of that particular man. When we consider knowledge, as a set of propositions, in abstraction from the knower, we make an abstraction that may easily lead us into error. The degree to which the individual thinker is active in his thinking determines the degree to which he feels and acts as the free man feels and acts. The whole sequence of his activity has to be known, if his claim to knowledge and understanding is to be assessed, and there is no short cut to this assessment through the postulation of independent objects of knowledge called 'propositions'. Spinoza's 'ideas' are phases of the activities of individuals, and truth, as a property of ideas, attaches to a sequence of thought through the manner of its organisation. There are no independent objects of thought in a neutral realm of ideas or propositions, which are proper objects of study. That each man should actively follow in his own thoughts the rational order

that is intrinsic to them is the lesson both of the *De Intellectus Emendatione* and of the *Ethics*. This is his independence and his rationality. That truth is a property attaching to independently identified objects of thought called propositions: that I give or withhold assent to propositions at will: that emotions are among the passive data of consciousness, and that they are to be entirely distinguished from the thought that may accompany and inform them—these are some of the assumptions, now largely unquestioned, that must be discarded before the argument of the *Ethics* can even be stated intelligibly.

Professor Hallett has the advantage of not accepting, and of not even bothering to notice, the distinctions which have been commonplaces in philosophy since Kant, and he dismisses the terminology and doctrines of contemporary empiricism with a gesture of casual contempt. He can therefore develop the consequences of Spinoza's notions of causality and activity without trying to re-state the premisses. His method is to find metaphorical equivalents of the original scholastic notions. The effect is peculiar: the strictness and the tone of the original are altogether lost, and yet beneath the metaphor the original argument is still discernible. The reader is not given an interpretation of Spinoza, and even less an assessment of the validity of his arguments. It is impossible to recommend this book to anyone who is not already convinced that Spinoza's philosophy is, at least in part, intelligible and possibly true, or possibly nearly true. But anyone who, like the present reviewer, already believes that Spinoza's theory of freedom is possibly true, or possibly at least partly true, will find in Professor Hallett's eccentric and obscure advocacy a valuable warning against the too easy assimilation of Spinoza's freedom, and of Spinoza's determinism, to later and parallel doctrines of freedom and determinism. Everything that is peculiar to Spinoza is emphasised: the rational structure that is common to every rigorous argument on these topics disappears.

STUART HAMPSHIRE

Introduction to Logic. By PATRICK SUPPES. Princeton: D. Van Nostrand Company Inc. (London: Macmillan & Co.), 1957. Pp. xviii + 312. £2.

THE scope of this book is indicated by the following remarks in the author's introduction (p. xviii): "In the narrow sense, logic is the theory of valid arguments or the theory of deductive inference. A slightly broader sense includes the theory of definition. A still broader sense includes the general theory of sets. Moreover, the theory of definition together with the theory of sets provides an exact foundation for the axiomatic method, the study of which is informally considered part of logic by most mathematicians."

The book is in two parts. Part I deals with first-order logic and with definitions. By first-order logic is meant the truth-functional logic and the logic of first-order predicates and of identity. This is ground covered in many other textbooks but several features of Professor Suppes's treatment deserve special attention.

(i) There is a detailed account of the notion of an interpretation of a formula, as used, *e.g.* in proofs of independence. A definition is given and a list of rules to be followed in finding interpretations of formulae of different kinds. This is an exceedingly useful section. However on one point greater explicitness is required. We are told in this section (4.2) that in an interpretation of a formula containing a free variable the free

variable should be replaced by a proper name. Now later in the book Professor Suppes mentions that it is customary in mathematics to omit initial universal quantifiers whose scopes extend over the remainder of a formula; thus: (*a*) $x + 0 = x$, may be written instead of (*b*) (x) $(x + 0 = x)$. According to Professor Suppes's usage in this case the x in (*a*) is called a free variable. But a distinction has to be drawn between the kind of free variable which is found in a formula which expresses a genuine statement (*e.g.* the x in (*a*) where (*a*) is intended as an abbreviation of (*b*)) and the kind of free variable which is found in a formula which is only a quasi-statement (*e.g.* the x in $x > 2$, where this formula is not being used to make the assertion that all numbers are greater than 2). What Professor Suppes says about free variables in section 4.2 is applicable, if I am right, only to free variables of this latter kind. I think that this should have been clearly stated in the text. Error may be avoided if before applying the instructions of section 4.2 we prefix to abbreviated statements such as (*a*) any initial universal quantifiers which the sense requires. (ii) An interesting analysis is given of the conventions of informal proof in mathematics. (iii) The procedure commonly known as existential instantiation, and called here existential specification, is managed in an efficient way involving what is claimed to be a new device, that of ambiguous names.

Chapter 8 on definition contains material absent from most textbooks. Two criteria are asserted for good definitions in a formal theory, eliminability and non-creativity. Eliminability requires no comment; non-creativity means that no theorem not containing the definiens should be derivable from axioms together with the definition unless it is also derivable from the axioms alone. Explicit rules for definitions of different kinds are given and the chapter concludes with an account of Padoa's principle which provides a method of proving that one primitive term is independent of, *i.e.* cannot be defined by means of, others.

Part II deals with elementary intuitive set theory. A clear account is given of the basic concepts and there are good chapters on relations and functions. The final chapter has the object of showing how theories which it would be impracticable to axiomatize within first-order logic may be axiomatized within set-theory by defining predicates in terms of notions of set theory. Two major examples are given, one concerned with probability and the other with mechanics. The subject-matter of this chapter should be of great interest to students of the philosophy of science. It must be added however that in this chapter the pace of the argument relative to its difficulty seems to show a sharp acceleration: a very much greater effort is required by the reader than in the rest of this highly instructive and interesting book.

J. A. Faris

Determinism and Freedom in the Age of Modern Science. Edited by Sidney Hook. (Proceedings of the first annual New York University Institute of Philosophy.) New York University Press, 1958. Pp. xv + 237. $5.00.

This book consists of ten papers and seventeen discussions from a total of twenty-five participants; so wide a symposium on a single topic seems a good idea, and we may wish the Institute many happy returns.

The title suggests a central question: how far do the findings of modern science in fact have any relevance to human freedom? In his opening

paper, Professor Blanshard, though a determinist on general grounds, makes it clear that for him there is no inference from "the unstable behaviour of single particles" (even if the physicists are right about this) to the indeterminism of a highly complex act such as that of human choice—and is it in any case (we may ask) instability that the libertarian wants? The four contributions specifically devoted to determinism in science (of which Professor Bridgman's judicious survey of the field is to a layman the most illuminating) do not venture outside the interpretation of modern physical theory, which is good policy. Lack of space and expert knowledge alike forbid detailed comment on these highly technical matters; but the remark may perhaps be ventured that the notion of probability derives its very meaning from the epistemological sphere of opinion and evidence (*cf.* Bridgman's approving quotation from Max Born, p. 53), and not even Professor Northrop's somewhat dogmatic assertions (*cf.* pp. 194-195) enable me to attach meaning to the phrase 'ontological probability'.

Physics apart, we have three general discussions of determinism by Brand Blanshard, Max Black and William Barrett. Blanshard's main point, in a suggestive paper, is that determinism should be seen in the light of various levels of causality, of which the highest is that rational determination which involves "control by immanent ideal". But surely this is not, as Blanshard hopes, enough for the indeterminist. The crux of the moral choice is precisely that a man *chooses whether to allow himself* to be determined by the rational ideal, and a determination which one can elect is a pretty equivocal sort of determination for a determinist to rely upon.

Max Black gives an interesting and careful analysis of the concept of 'making something happen'; but as with all paradigm-case arguments (I sympathise with Mr. Watkins against Professor Flew) it is not clear to me that this proves more than that this is the way we talk. And I am not sure that we can dismiss the thesis of universal causality simply by remarking that the term 'causality' in such a use is too generalised and schematic for rational discourse—though doubtless it is difficult to define.

Finally in this section William Barrett explores the realms of mathematics, physics, art and history to find evidence of unpredictability, and seems amply justified in his conclusion that "as a total thesis determinism simply remains to be proved".

In the sphere of human conduct, we have first a fascinating attempt by H. L. A. Hart to justify the notion of legal responsibility in a way which cannot be upset by determinism. My only doubt is that voiced by Elizabeth Beardsley—whether the total separation of legal from moral responsibility which this entails is ultimately tenable.

The determinist case in ethics is strongly presented on psychological grounds by Paul Edwards and John Hospers. They both argue that man cannot choose what he shall will or desire, though he may be free to do what he wills or desires; in short, one cannot determine one's own character, from which one's actions flow. But Hospers holds, and perhaps Edwards would agree, that a man may still be called morally responsible provided his acts do not flow from *unconscious* or *external* compulsion and are amenable to rational argument. This watered-down version of moral responsibility is vigorously attacked by Howard Hintz.

It is in the analysis of choice that this book is most lacking; for there, if anywhere, the libertarian case stands or falls. There are plenty of contributors to point out that 'I could have done otherwise' may be completed by 'If I had wished'; but none of the anti-determinist writers provides a workable alternative to this obviously pro-deterministic

interpretation; though Richard Taylor hints at one when he says that what is needed is a view according to which every happening has a *reason* though some are *contingent*. Taylor's essay is an able and all-too-brief attempt to provide a plausible theory of free agency; but the truth is, as he well realises, that the concepts involved in the salvaging of moral responsibility are too much out of fashion to be introduced effectively in seven pages.

I have noticed a few trivial misprints, and two places where the sense seems to demand emendation of the text. On page 98, line 25, 'obtainable' should surely be 'unobtainable'; and on page 219, line 5 of paragraph 2, I read 'forge' for 'forgo'. True, this appears to make Paul Weiss advocate the forging of accounts; but *praestet lectio difficilior*.

On the whole, this is a worthwhile collection; and, unlike most collections, has a useful index. But few English philosophers will be able to afford it, unless they are very lucky—or determined.

NORMAN BROWN

Perceiving: A Philosophical Study. By R. M. CHISHOLM. Cornell University Press. (London: Oxford University Press). Pp. 197. 22s. net.

PROFESSOR CHISHOLM'S book, which is composed with admirable clarity and care, consists of three main parts. In Part I, *The Ethics of Belief*, he is concerned to define and discuss such notions as evidence, acceptability, knowledge, certainty, probability: in Part II, *Evidence*, he discusses (and invents) some uses of "appear", considers some questions about the justification of perceptual claims, and some more general questions about criteria for the "adequacy" of evidence: in Part III, *The Objects of Perception*, he deals with "sensing", with secondary qualities, with some problems about the perception of objects, and with the question whether "intentionality" can be eliminated from his battery of perceptual concepts. (He decides that, perhaps not surprisingly to many, it cannot.) There is also an appendix on Phenomenalism, but as this does little more than point out that the phenomenalist thesis is inconsistent with certain theses advanced earlier in the book, it is of no great independent importance.

There seems to me to be a legitimate question about the method of proceeding which Professor Chisholm mainly adopts. He proposes from time to time a number of definitions with the aim, as he says, of showing that "by restricting ourselves to these ways of talking, we avoid the philosophical questions to which the use of ordinary locutions seems to give rise". Briefly, this does not sound like a method of answering philosophical questions; and it is also non-committal on the interesting question whether "ordinary locutions" puzzle because they are apt to be misconstrued, or whether they are in some way actually defective and wrong. Professor Chisholm's method would, I take it, be manifestly pointless if his proposed ways of talking seemed arbitrarily contrived; for they would then amount to a simple refusal to discuss pre-existing problems at all, to a change of subject in fact. This, fortunately, is not the present case. Most of his definitions do seem to come close to what one had antecedently supposed the word in question to mean, and are thus clearly relevant to what one had supposed the problems to be. But there are oddities. "Certain" is so defined that, very surprisingly, a man may

not—even, usually, will not—be certain of something that he knows and even sees to be the case; "reasonable", in such a way that it is perfectly reasonable for a man to accept a proposition as to which he has, and knows he has, no evidence whatever; and "perception" itself is so defined (p. 150) that "there are people who perceive things, who see, hear, feel, or touch them, without *knowing* that they are perceiving anything, without knowing that they are seeing, hearing, feeling, or touching anything". It is not certain that such oddities are damaging in their contexts; but they do raise the question what is to control the proposal of ways of talking, beyond the desire to "avoid philosophical questions"—a desire which, after all, can be easily gratified in other ways.

The crux, I believe, of Professor Chisholm's argument is reached in section 6, "On Some Marks of Evidence". He there says that, by the "empirical criterion", the state of a man who is "being appeared to" in a particular way constitutes "adequate evidence" for him that he is being appeared to in that way. Empiricism, or at least an extreme version of it, he then defines as the thesis that the empirical criterion is the *only* criterion of evidence. But if so, he argues, no ordinary perceptual claim could be shown to be so much as probable. Therefore, the empirical criterion must be supplemented by the further criterion that we "have evidence for all of our *sensible takings*". A sensible taking is, roughly, taking some thing to be characterized by a sensible quality—"sensible" qualities are listed but not defined—and the suggestion is that our sensible takings, when they occur, themselves constitute "adequate evidence" for what we so take. All this is, I think, a remarkable way of saying that, if we are to be justified in holding those beliefs about the world which we suppose ourselves to be justified in holding, then we must be entitled to offer and accept without evidence (as "basic" perhaps) not only assertions about how things appear, but at least some assertions about how things are. (To say that something is accepted *without* evidence seems to me to be the same as to say that the accepting is itself the "evidence" for what is accepted.) Professor Chisolm is less confident that his further criterion, with the empirical criterion, is sufficient, than he is that the latter is insufficient by itself; at this point he is very cautious indeed. But if what he cautiously says is exposed to complaint, I think that the complaints should be (1) that his further criterion, in default of closer examination of what is meant by "sensible", is not perfectly clear; and (2) that denial of the sufficiency of the empirical criterion is perhaps less interesting than enquiry into the reasons why its sufficiency has so often been asserted. There is also a point that might have been further considered here. Professor Chisholm's contention is that, if any statement of how things appear is to support any statement of how things are, it has to be conjoined with a statement of "independent information" about how things are. Now this is true of 'appearance-statements' taken singly, or even in small batches; but what about large—indefinitely large—conjunctions of them? This surely alters the logical picture: perhaps it is so contrived and unrealistic as to destroy it altogether, leaving everything in mere confusion; but is it not this, all the same, that the devotee of "empiricism" would insist upon discussing?

Of the many other discussions in the book those on "Three Uses of Appear Words" and "Secondary Qualities" may be found the most interesting. But the argument throughout, though often conducted in language of extreme artificiality, is always clear, and never idle.

G. J. WARNOCK

Therapeia: Plato's Conception of Philosophy. By ROBERT E. CUSHMAN. Chapel Hill: The University of North Carolina Press. Pp. xxii + 322. $6.00.

MR. CUSHMAN'S work is neither a commentary on Plato's dialogues nor an examination of specific philosophical difficulties associated with the Platonic thought. It is rather a long and distressingly repetitious argument in support of a curious claim—the claim, roughly, that an "existentialist" approach to Platonic doctrines is both warranted and desirable. The plausibility of this contention derives from an early characterization of Plato's conception of Philosophy as a "way of life, a way out of chaos in human existence" (p. xv); and its cogency rests on the alleged Platonic distinction between "truth *about* reality" and "truth *as* reality" (p. xviii). Once this is granted, the subsequent clarification that for Plato "decisional truth" (p. xix) is the proper object of philosophy, follows naturally; and, similarly, the supplementary claim that metaphysical principles are derivable from "axiological experiences", cannot be resisted. But no sooner do we allow these to pass and the apparently innocuous observation that man's plight shows that he has been "cut loose from . . . metempirical rootage and . . . dispossessed of all trans-phenomenal responsibility" (p. xvi), unexpectedly gets us into an impossible situation. Henceforth, if for no other reason but for mere consistency, we are forced to acknowledge the centrality of the notion of *θεραπεία* and admitting this we cannot but agree with Mr. Cushman that Plato's philosophy is essentially "soteriological" (p. 285) and ought to be explained and judged in terms of the alleged programme implicit in this concept. Following through patiently the less obvious consequences of the position, we are tactfully persuaded to agree that Socrates thought of human intelligence as "inveterately axiological" (p. 25) and of virtue as the "condition of knowledge" (p. 71), and, similarly, we are given reasons to believe that the Idea of the Good can be exhibited through "*eduction*" (p. 215) and knowledge of ultimate reality through "self-authentication" (p. 236). In a parallel fashion, we are gently induced to admit that Plato's ontology involves a "scale of Being" (p. 248) and we are reminded that for Plato truth and reality are "*boniform*" (p. 154). And this goes on and on as Mr. Cushman takes up systematically one platonic concept after another (*e.g. ἀνατροπή, ἀνάμνησις, ἀρετή. δόξα, ἔρως ῥητορική, διαλεκτική*) and submits it to his "existentialist" reappraisals. The end result of the whole adventure is disappointing. We may have been perhaps intrigued with the method by which one could, with considerable logical impunity, attribute to Plato notions and principles inconceivable within the framework of Greek thought, but we have learned very little about Plato himself. Neither the problems that compelled him to say what he said, nor the supposed solutions he offered, seem any clearer at the conclusion of this discussion than at its beginning. Admittedly, there is something seductive in the idea of making Plato look neat and remain intelligible. But the variety of the types of conceptual problems encountered in Plato's works does not admit of such wholesale treatments and soon the same issues re-emerge as irresoluble and paradoxical. Mr. Cushman quite ingeniously selects and tailors his evidence to fit the formal requirements of his *a priori* argument; but, obviously, this does not constitute either an interpretation of this evidence or a proof of its logical adequacy. Part of the weakness of this approach could be attributed to Mr. Cushman's reluctance to give us substantial quotations from the texts. His exclusive reliance on copious

cross-references, aside from establishing the author's erudition, do very little to help the reader to grasp the arguments of the text.

The book is full of misprints and many Greek words have been misspelled. The principle followed in the transliterations is unclear.

PETER DIAMADOPOULOS

Philosophy and Argument. By HENRY W. JOHNSTONE, Jr. The Pennsylvania State University Press, 1959. Pp. 141. $4.00.

THIS is an enquiry into the kinds of disagreement peculiar to philosophy, of the kinds of arguments used to settle these disagreements, and of the standards of validity and of truth to which the protagonists may legitimately appeal. Besides frequent short examples from the works of various philosophers, a chapter is devoted to arguments used by Hume concerning causal necessity. The book ends with certain conclusions about the nature of man which are said to follow from the author's thesis about what kind of argument is valid in philosophy.

This thesis is that 'all valid philosophical arguments are addressed *ad hominem*'. One reason for this view seems to be the belief that a philosophical statement or system is one which expresses the holder's commitment, whether conscious or unconscious, to a way of life or a basic position about it. Hence everyone is said to have a philosophical position; philosophical disagreement is personal and moral and involves opposite commitments; and philosophical argument is designed to support or attack a position rather than to prove or disprove its truth. But no defence is given of this view of philosophy and no discussion occurs of alternative views. This is a serious omission, for obviously views about the kinds of arguments peculiar to philosophy and views about the nature of philosophy are interdependent. There are many views about the nature of philosophy for which the author's thesis is implausible; and, indeed, many of his examples of 'philosophical statements' are very dubious.

A second related reason for the author's thesis follows from his view that the truth of a philosophical statement is relative to the argument for it, and that opposing arguments might establish a philosophical statement and its contradictory as both true. Therefore, arguments intended to dispute the *truth* of a philosophical statement will necessarily and sufficiently dispute the *arguments* advanced in its favour. Now the author's case for this dependence of argument and truth seems to be simply a mistaken deduction from the two plausible views that 'the argument for a philosophical statement is always a part of its *meaning*' and 'we cannot think of a statement as *true* without knowing its meaning'. That the author has confused the relation of the arguments for a statement to its meaning and the relation of these arguments to its truth would account for his denial (p. 32) of the interdependence of argument and meaning in scientific statements because of his denial of the interdependence of argument and truth there (p. 23).

The author has done a service in stressing the primary part played by argument in philosophy, but has not, in my opinion, fathomed the reasons for this. One reason, I think, why much philosophy consists in attacks on one's opponent's arguments is not that the truth of his statement depends on them, but because his conclusions so shamelessly fly in the face of what we know to be so that what interests us is the arguments which led him to these conclusions. This was why Moore so commonly used '*ad*

hominem' arguments. Another reason lies in the 'analytic' nature of the subject.

By an '*argumentum ad hominem*' the author means one which shows that the attacked person, or his argument, has defeated his own purpose. The classification of such *ad hominem* arguments includes, therefore, charges of unintelligibility, of dogmatism, of ineffectiveness, of self-disqualification, of *tu quoque*, etc. Although interesting examples of all these are provided from the works of philosophers—and many more might have been provided from Moore's writings—the author does not, and could not, show that they are restricted to philosophy nor philosophy to them; and the attempts made to show they are characteristic of philosophy seem to me quite inadequate. These arguments are, in fact, the stock in trade of every college debater and parliamentarian. This tendentiousness of some of the author's examples, together with a certain looseness of argument at the critical points of the thesis, gives an air of eccentricity to the whole.

ALAN R. WHITE

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IX.—NOTES

JOINT SESSION WITH THE ARISTOTELIAN SOCIETY

The next General Meeting of the Mind Association and Joint Session with the Aristotelian Society will be held at Exeter University from 8th to 11th July 1960. Notices and application forms will be sent automatically only to members of the Association resident in Europe (List (a) at the end of this issue). If any member on List (b) expects to be in England at that time and wishes to attend the Session, he is asked to write to the Treasurer, Mr. J. D. MABBOTT, at St. John's College, Oxford, before the end of April and papers will be sent him.

Any member who is unable to attend but would like to obtain the volume of papers to be read at the Joint Session should write to the publishers for the volume in July (Messrs. Harrison and Sons Ltd., 45 St. Martin's Lane, London W.C.2).

Back Numbers. The Philosophical Institute at Göteborg wishes to obtain a series of back numbers of MIND previous to 1952. Anyone who wishes to dispose of such a series should write to M. FURBERG, Filosofiska Institutionen, Södra Vägen 46, Göteborg, Sweden.

Dear Sir,

We should be obliged if you would draw the attention of your readers to a proposal to form a British Society of Aesthetics. This is considered desirable, both generally to promote the study of the subject, including communication with foreign correspondents, and with a particular view to the forthcoming International Congress of Aesthetics to be held in Athens from September 1st to September 6th, 1960. Those of your readers who are interested should write to the Acting Secretary at the Library, University College, Singleton Park, Swansea.

Yours etc.

F. J. W. HARDING

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In the recently published volume, *Logical Positivism*, edited by A. J. Ayer, the name of the translator of two articles is inaccurately stated. Rudolf Carnap's "Psychology in Physical Language" and Otto Neurath's "Protocol Sentences" were translated by Dr. Frederic Schick of Columbia University and not by George Schick. The publishers hereby wish to apologize to Dr. Schick for this mistake.

Would Mr. R. C. Skinner tell the Editor his new address?

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HALL (C.), Flat 2, 10 South Hill Park Gardens, London, N.W.3.
HALL (R.), Philosophy Department, Queens College, Dundee, Scotland.
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JØRGENSEN (Prof. J.), Parkvaenget 15, Charlottenlund, Denmark.
JOSEPHS (S. A.), 35 Bodley Road, Littlemore, Oxford.

KÄHLER (Hr. J.), Hostrupsvej 11-4, Copenhagen V, Denmark.
KASSMAN (A. A.), 17 Lymington Road, London, N.W.6.
KEENE (G. B.), Philosophy Department, University College of N. Staffs, Keele, Staffs.
KEMP (J.), Department of Moral Philosophy, The University, St. Andrews, Fife.
KENNEDY (Miss M. R.), 26 Lygon Road, Edinburgh 9.
KENNY (Rev. A.), 315 Iffley Road, Oxford.
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KNOX (Principal T. M.), The University, St. Andrew's, Fife.
KOLNAI (A.), 24 Hilltop Road, London, N.W.6.
KÖRNER (Prof. S.), Department of Philosophy, The University, Bristol 2.
KOVESI (J.), Moral Philosophy Department, The University, Edinburgh.
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LASLETT (P.), Trinity College, Cambridge.
LAU (D. C.), School of Oriental and African Studies, University of London, London, W.C.1.
LAWTON (Dr. C. L.), c/o York County Savings Bank, St. Helen's Square, York.
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LEVETT (Miss M. J.), Dial Cottage, Rolvenden Layne, near Cranbrook, Kent. (*Life Member.*)
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LEWY (Dr. C.), 49 De Freville Avenue, Cambridge.
LEYDEN (W. von), 5 Pimlico, Durham.
LEYS (C.), Balliol College, Oxford.
LIDDELL (A. T. W.), 10 Dornoch Avenue, Giffnock, Glasgow.
LILLIE (Dr. R. A.), 267 Braid Road, Edinburgh.
LING (J. F.), 15 Swan Street, Stourbridge, Worcs.
LITTLE (Mrs. H. E. S.), Westbank, Newburgh Road, Wormit, By Dundee, Scotland.
LLOYD (Prof. A. C.), Philosophy Department, The University, Liverpool 3.
LUCAS (P. G.), 28 Watson Road, Sheffield 10, Yorks.
LUCE (Rev. Canon A. A.), Ryslaw, Bushy Park Road, County Dublin, Ireland.
LUNDBERG (Hr. K.), Frederiksbergallé 94 II, Copenhagen V, Denmark.
LUNN (Prof. Dr. V.), Rigshospitalets Psykiatriske Afd., Copenhagen Ø, Denmark.
LUSCHEI (E. C.), University College, Oxford.

MABBOTT (J. D.), St. John's College, Oxford. (*Life Member.*)
MACBEATH (Prof. A.), Lorne Cottage, Aberfeldy, Perthshire.
MCCARTHY (Rev. A.), 1 Northbrook Road, Dublin, S.W.8, Ireland.
MCCRACKEN (Dr. D. J.), The University, Edmund Street, Birmingham.
MCDERMOTT (F. E.), 16 Queen's Parade, Grimsby, Lincs.
MACGILL (N. W.), Huntroyd, 38 Manchester Road, Denton, Manchester.
MCGRATH (P.), Killerig, Carlow, Ireland.
MCGUINNESS (B. F.), Queen's College, Oxford.
MACINTYRE (A. C.), Bedlington House, Potternewton Mount, Leeds 7.
MACIVER (A. M.), The University, Southampton.
MACKAY (D. M.), Physics Department, King's College, Strand, London, W.C.2.
MACKENZIE (P. T.), St. John's College, Cambridge.
MCKIE (J. I.), Brasenose College, Oxford.
MACLAGAN (Prof. W. G.), 6 University, Glasgow, W.2.
MACLENNAN (Prof. R. D.), Inverinate, By Kyle, Ross-shire, Scotland.
MACMILLAN (J. H.), New Park House, Ampleforth College, Yorks.
MACMURRAY (Prof. J.), The University, Edinburgh.
MACNABB (D. G. C.), Pembroke College, Oxford.
MCNEILLY (F. S.), Moral Philosophy Department, The University, St. Andrew's, Fife, Scotland.
MCPHERSON (T.), Department of Philosophy, University College, Bangor, N. Wales.
MALPAS (R. M. P.), Moor Lodge, Oldfield Road, Heswall, Cheshire.
MANSER (A. R.), 190 Kimbolton Road, Bedford.
MARSH (D. R.), 25 Albury Drive, Pinner, Middlesex.
MARSH (Rev. J.), Mansfield College, Oxford.
MARSHALL (G.), 27 Arnold Avenue, Blackpool, Lancs.
MATTHEWS (Miss G. M.), University College, Bangor, N. Wales.
MATTHEWS (W. E.), Oldham's Hall, The Schools, Shrewsbury.
MAYER (D. C.), 13 Dalkeith Road, Harpenden, Herts.
MAYO (B.), The University, Edmund Street, Birmingham 3.

MAYS (Dr. W.), Department of Philosophy, University of Manchester, Manchester 13.
MEAGER (Miss R.), 16 Southdean Gardens, London, S.W.19.
MELLOR (W. W.), c/o Westminster Bank, 121 High Street, Oxford.
MIDGLEY (Mrs. M.), 3 Lyndhurst Gardens, Newcastle-on-Tyne 2.
MIKKELSEN (Fr. K.), Nørrevold 22, Copenhagen K, Denmark.
MILES (R. T.), 26 Central Road, Bromsgrove, Worcs.
MILES (T. R.), Clwyd-y-Wylan, Menai Bridge, Anglesey, N. Wales. (*Life Member.*)
MILL (Miss J. M.), Craag, Kinlochmoidart, Fort William, Inverness-shire.
MILLIGAN (D. E.), 9A Hermitage Drive, Edinburgh 10.
MILNE (Dr. A. J.), 28 Adelaide Road, Belfast.
MINERS (N. J.), 60 Finlay Street, Fulham, London, S.W.6.
MINIO-PALUELLO (Dr. L.), 22 Polstead Road, Oxford.
MINKUS-BENES (P.), 9 Guildford Street, London, W.C.1.
MITCHELL (A. H. M.), Carrick, 10 Wolsey Road, Moor Park, Northwood, Middlesex.
MITCHELL (D.), Worcester College, Oxford.
MØLLER (Fr. L.), Svinget 4 (5. Sal), Copenhagen S, Denmark.
MONINS (I. R.), Ringwould House, Ringwould, near Dover, Kent. (*Life Member.*)
MONTAGUE (R. D. L.), 1 Dempster Terrace, St. Andrew's, Fife, Scotland.
MONTEFIORE (A. C.), University College of N. Staffs., Keele Hall, Stoke-on-Trent.
MOORE (R. B. W.), 8 St. Pauls Road, Cambridge.
MORITZ (Prof. M.), Vintergatan 5 C, Lund, Sweden.
MORRIS-JONES (H.), University College, Bangor, N. Wales.
MOSS (J. M. B.), 9 Jesus Lane, Cambridge.
MOTT (C. F.), 10 Sedley Taylor Road, Cambridge.
MOUSTGAARD (Hr. I. K.), Vestre Kirkegaards Alle 24 (4. Sal), Copenhagen SV, Denmark.
MUNCH-PETERSEN (Hr. J.), Carl Plougsvej 6 III, Copenhagen V, Denmark.
MUNDLE (Prof. C. W. K.), Department of Philosophy, University College, Bangor, N. Wales.
MURE (G. R. G.), Merton College, Oxford.
MURPHY (D. P. A.), 20 Hamilton Road, Cowley, Uxbridge, Middlesex.
MURRAY (J.), 62 Porchester Gate, London, W.2.

NAMOOS (G. R.), 44 Minto Street, Edinburgh 9.
NEWFIELD (J. G. H.), 13 Endsleigh Street, London, W.C.1.
NICHOLSON (Mrs. C. E.), 10 Salisbury Road, Harrow, Middlesex.
NISSEN (Mag. I), Cort Adelers Gate 12, Oslo, Norway.
NOALL (A. W.), 8 Winton Road, Victoria Docks, London, E.16.
NOLET (Rev. Y.), The Old Palace, St. Aldates, Oxford.
NOWELL-SMITH (Prof. P. H.), White House, North Avenue, Leicester.

O'CONNOR (Prof. D. J.), Philosophy Department, The University, Exeter.
OPPENHEIMER (Lady), 10 Northmoor Road, Oxford. (*Life Member.*)
O'SHAUGHNESSY (B.), 10 Welbeck Mansions, Inglewood Road, London, N.W.6.
OTTAWAY (A. K. C.), Beech Cottage, Chapel Street, Halton, Leeds 15.
OVEREND (R. G.), 198 Grange Loan, Edinburgh 9.
OWEN (G. E. L.), 11 Beech Road, Headington, Oxford.

PAGE (E.), 99 Woodstock Road, Oxford.
PARKINSON (G. H. R.), Philosophy Department, The University, Reading, Berks.
PARR (J. A. D.), 239 Main Road, Sidcup, Kent.
PARSONS (E. S.), Via Acqua Traversa 218, Rome (951), Italy.
PARSONS (Rev. R.), The Rectory, Acton, London, W.3.
PATON (Prof. H. J.), Nether Pitcaithly, Bridge of Earn, Perthshire.

PAUL (G. A.), University College, Oxford.
PAULSEN (Hr. H.), Gl. Kongevej 70, Copenhagen V, Demark.
PEARS (D. F.), Corpus Christi College, Oxford.
PEETZ (D. W.), 39 Coates Gardens, Edinburgh 12.
PENMAN (J. A.), Gateletts, Culgaith, Penrith, Cumberland.
PERRY (J. W.), 19 Newark Way, Hendon, London, N.W.4. (*Life Member.*)
PETERS (R. S.), Birkbeck College, Malet Street, London, W.C.1.
PILKINGTON (G. W.), Psychological Laboratory, The University, Sheffield 10.
PINKERTON (R. J.), 5 St. John's Road, Oxford.
PLACE (U. T.), Corpus Christi College, Oxford.
PLEYDELL-PEARCE (A. G.), Department of Extra-Mural Studies, The University, Edmund Street, Birmingham 3.
POLE (D. L.), Philosophy Department, King's College, Strand, London, W.C.2.
POLLITT (A.), Carnethy, York Road, Deganwy, Carnarvonshire.
PONTING (K. G.), Becketts House, Edington, near Westbury, Wilts.
POOLE (H. W.), 339 Westbourne Grove, Westcliff-on-Sea, Essex.
POPPER (Prof. K. R.), London School of Economics, Houghton Street, London, W.C.2.
PORTEOUS (Prof. A. J. D.), 3 Kingsmead Road North, Birkenhead, Cheshire.
POWELL (Miss B.), 11 Richmond Road, Swansea, Glam.
PRESTON (L. F.), 84 Kenilworth Road, Ashford, Middlesex.
PRICE (Prof. H. H.), New College, Oxford.
PRICE-WILLIAMS (D. R.), London School of Economics, Houghton Street, London, W.C.2.
PRIOR (Prof. A. N.), Philosophy Department, The University, Manchester 13.

QUINN (D.), 62 West Crescent, Darlington.
QUINTON (A. M.), New College, Oxford.

RALLS (A. E.), Merton College, Oxford.
RAMSEY (Rev. Canon I. T.), Oriel College, Oxford.
RANKIN (H. D.), Latin Department, The University, Sheffield 10.
RAPHAEL (D. D.), Department of Moral Philosophy, The University, Glasgow.
RASMUSSEN (Fr. D. M.), Fensmarkgade 65, Copenhagen N, Denmark.
RASMUSSEN (Prof. E. T.), Skovtoftebakken 13, Lyngby, Denmark.
REES (Dr. D. A.), Jesus College, Oxford.
REES (Prof. J. C.), University College, Swansea, Glam.
REES (W. J.), Philosophy Department, The University, Leeds 2.
RÉGNIER (Rev. M.), Archives de Philosophie, Vals près Le Puy, Haute Loire, France.
REID (A. D.), 211 Niddrie Mains Road, Craigmillar, Edinburgh 9.
REID (Prof. L. A.), Institute of Education, University of London, Malet Street, London, W.C.1.
RENOWDEN (Rev. C. R.), St. David's College, Lampeter, Cards., Wales.
RENSHAW (E.), 64 Everest Avenue, Llanishen, Cardiff, S. Wales.
RENTON (G. P.), 15 Dalewood Avenue, Beauchief, Sheffield 8.
RHEES (R.), 64 Eaton Crescent, Uplands, Swansea, Glam.
RICHARDSON (C. A.), 9 Orchard Lane, West Wimbledon, London, S.W.20.
RICHES (P.), Via Pietro Maestri 2, Milano, Italy.
RIDDIFORD (D. G.), Brentwood School, Essex.
RIDGWELL (P. J.), 77 Great Cambridge Road, Cheshunt, Herts.
RITCHIE (Prof. A. D.), Old College, The University, Edinburgh.
ROBERTS (J. W.), 1 Higham Lane, Tonbridge, Kent.
ROBINSON (G. S.), 102 Alma Road, Southampton.
ROBINSON (RICHARD), Oriel College, Oxford.
ROOT (Rev. H. E.), Emmanuel College, Cambridge.
ROSS (Prof. A.), Mundtsvej 10A, Lyngby, Denmark.
ROSS (Sir W. D.), 17 Bradmore Road, Oxford.
ROSSI-LANDI (F.), Via N. Sauro 6, Milano, Italy.
ROTH (Prof. H. L.), 3 Merton Street, Cambridge.

ROXBEE COX (J. W.), 3 Upper Harley Street, London, N.W.1.
RUSSELL (Rt. Hon. Earl), Trinity College, Cambridge.
RUSSELL (Prof. L. J.), 304 Hagley Road, Edgbaston, Birmingham 17.
RUSSON (L. J.), 22 Kingsgate Road, Winchester, Hants.
RYLE (Prof. G.), Magdalen College, Oxford.
RYLE (M. T.), Public Bill Office, House of Commons, London, S.W.1.

SALMAN (H.), Le Saulchoir, Soisy-sur-Seine, Seine et Oise, France. (*Life Member.*)
SAW (Miss R. L.), 72 Grosvenor Avenue, Carshalton, Surrey.
SAWERS (R.), Lye Green Cottage, Claverdon, Warwickshire.
SCHAPER (Dr. Eva), Logic Department, The University, Glasgow, W.2.
SCHOSZBERGER (Dr. H.), Thorneycroft, Coton Hill, Shrewsbury, Salop.
SCHULTZER (Prof. B.), Taarbaek Strandvej 56a, Klampenborg, Denmark.
SCHWYZER (H. R. G.), Philosophy Department, The University, Reading, Berks.
SCOTT (J.), 9 Mertoun Place, Edinburgh 11.
SCOTT (Prof. J. W.), Windylaw, Lisvane, Cardiff.
SCOTT-TAGGART (M. J.), King's College, Cambridge.
SELIGMAN (P.), 27 Park Drive, Rayners Lane, Harrow, Middlesex.
SHEARN (M.), Flat 3 Dagmar House, 420 Woodborough Road, Nottingham.
SHEPHERD (B. E.), 7 Cotman Gardens, Edgware, Middlesex.
SHUTTLEWORTH (A.), 120 Heathbank Road, Cheadle Hulme, Cheshire.
SIMOPOULOS (J. C.), Magdalen College, Oxford.
SINGLETON (Miss W. A.), 35 Wimborne Drive, Pinner, Middlesex.
SKEMP (Prof. J. B.), 9 St. Nicholas Drive, Whitesmocks, Durham.
SLOMANN (Hr. A.), Trommesalen 7, Copenhagen V, Denmark.
SMART (R. N.), Philosophy Department, King's College, Strand, London, W.C.2.
SMILEY (P. O'R.), Glyn Dyfi, Caradog Road, Aberystwyth, Cards.
SMILEY (T. J.), Clare College, Cambridge.
SMITH (R. N. W.), Monymusk, Largo Road, St. Andrew's, Fife.
SMITHERS (D. L.), 143 Marsham Court, Marsham Street, London, S.W.1.
SØRENSEN (Hr. C. E.), Søllerød Park Blok 5, nr. 12, Holte, Denmark.
SPILSBURY (R. J.), 54 Blacket Place, Edinburgh 9.
SPRIGGE (T.), 101 Lexham Gardens, London, W.8.
SPRY (G.), 28 Chester Street, Belgrave Square, London, S.W.1.
STERN (Dr. Axel L.), 210 Victoria Avenue, Hull, Yorks. (*Life Member.*)
STERN (K.), 16 Horbury Crescent, London W.11.
STIGEN (A.), Drammensveien 91b, Oslo, Norway.
STOOTHOFF (R. H.), 17 St. John's Street, Oxford.
STOPES-ROE (Dr. H. V.), Peaveley House, Haslingfield, Cambs.
STRANG (Hon. Colin), Philosophy Department, King's College, Newcastle-on-Tyne 1.
STRAWSON (P. F.), University College, Oxford.
STRØMGREN (Prof. E.), Risskov, Århus, Denmark.
STUTCHBURY (O. P.), Gayles, Friston, near Eastbourne, Sussex.
SUHR-JESSEN (Hr. H. J. F.), Sct. Thomas Alle 4, Copenhagen V, Denmark.
SULLIVAN (D. D. H.), 1 Harcourt Buildings, The Temple, London, E.C.4.
SUMMERSBEE (S.), 11 Balcaskie Road, Eltham, London, S.E.9.
SUTTON (C. W. H.), St. Peter's Hall, Oxford.
SZAFRAN (J.), The University, Exeter.

TANNER (M. K.), 44 Warwick New Road, Leamington Spa, Warwickshire.
TAYLOR (Mrs. M. H.), Shincliffe Hall, Durham.
TAYLOR (W.), 14 Wade Court Road, Havant, Hants.
TEALE (Prof. A. E.), University College, Keele, Staffs.
THOBURN (J. W.), 27 Queen's Court, Newcastle-on-Tyne 4.
THOMAS (D. O.), Coleg Harlech, Merioneth, Wales.
THOMAS (Rev. J. H.), Theology Faculty, The University, Manchester 13.
THOMAS (L. E.), Glyn Cottage, Belmont Road, Bangor, Carnarvonshire.
THOMPSON (J. R.), 5 Barbara Avenue, Scraptoft Lane, Leicester.

THOMPSON (Rev. W. M. D.), The Manse, Oxnam, Jedburgh, Roxburgh-shire, Scotland.
THOMSON (J. F.), Corpus Christi College, Oxford.
TIMMERMANN (Dr. E.), Gl. Kongevej 140, Copenhagen V, Denmark.
TOBIAS (S. A.), Engineering Laboratory, Trumpington Road, Cambridge.
TOMS (E.), 72 Thornbridge Road, Garrowhill, Baillieston, near Glasgow.
TORDRUP (Hr. S. A.), Hyldegaardsvej 22, Charlottenlund, Denmark.
TOULMIN (Prof. S. E.), 45 Cliff Road, Leeds 6.
TREE (Prof. R. J.), The College, Llandovery, Carms., Wales.
TUCKER (J. W.), University College, Bangor, N. Wales.
TULLOCH (Miss D. M.), Canterbury Hall, Cartwright Gardens, London, W.C.1.
TURNER (Rev. V. G.), Campion Hall, Oxford.

URMSON (J. O.), Corpus Christi College, Oxford.

VALLANCE (D.), 4 Braefoot Avenue, Milngavie, Glasgow.
VALPOLA (Dr. V.), Korsutie 38, Pitäjänmäki, Finland.
VARLEY (H.), 2 Fairfax Avenue, Didsbury, Manchester 20.
VESEY (G. N. A.), 44 Clock House Road, Beckenham, Kent.
VIVIAN (F.), Little Acre, Boxford, Colchester, Essex.
VON WRIGHT (Prof. G. H.), 4 Skepparegatan, Helsingfors, Finland.

WALDRON (C. F.), 22 Marton Grove Road, Middlesbrough, Yorks.
WALL (F. P.), Wahroonga, Green Dene, East Horsley, Surrey.
WALLBANK (Rev. Dr. N. E.), The Gatehouse, West Smithfield, London, E.C.1.
WALSH (W. H.), Merton College, Oxford.
WARBURG (Prof. E.), Frederik D. V's Vej 3, Copenhagen Ø, Denmark.
WARNER (D. H. J.), Philosophy Department, The University, Exeter, Devon.
WARNOCK (Mrs. G. J.), St. Hugh's College, Oxford.
WATKINS (J. W. N.), 11 Erskine Hill, London, N.W.11.
WATSON (W. N.), The Elms, Aston, Oxon.
WAYLAND (C. R.), c/o Fraulein A. B. Rombach, Hungerbergstrasse 11, Zurich 11, Switzerland.
WEBSTER (D.), Idlewild, Fountainhall Road, Aberdeen.
WERNHAM (A. G.), 19 College Street, St. Andrew's, Fife, Scotland.
WESTBY (G.), Psychological Laboratory, The University, Hull.
WHITE (A. R.), The University, Hull.
WHITEHOUSE (S. P.), The Parsonage, Dukinfield, Cheshire. (*Life Member.*)
WHITELEY (C. H.), 46 Tenbury Road, King's Heath, Birmingham 14.
WHITROW (Prof. G. J.), 6 Albert Mansions, Albert Bridge Road, London, S.W.11.
WICKEN (A. J.), 130 Rydens Road, Walton-on-Thames, Surrey.
WIGGINS (D. R. P.), 43 Christchurch Hill, London, N.W.3.
WIGHTMAN (S.), Danby House, Harthill, near Sheffield.
WILFORD (F. A.), 5 Park Close Cottages, Wick Lane, Englefield Green, Surrey.
WILKIE (R.), 17 Millbrae Crescent, Langside, Glasgow, S.2.
WILLIAMS (C. J. F.), 16 Bridge Street, Pershore, Worcs.
WILLIAMS (T. D.), Old Mill House, Combs, Stowmarket, Suffolk.
WILLIS (R. J.), 39 Sheen Common Drive, Richmond, Surrey.
WINCH (P. G.), University College, Swansea, Glam.
WINDROSS (B.), 24 Lawrence Road, Hove 3, Sussex.
WINDSOR (A. C. A.), c/o Messrs Cowan & Dalmahoy, W.S., 31 Charlotte Square, Edinburgh 2.
WISDOM (Prof. J.), Trinity College, Cambridge.
WISDOM (J. O.), The London School of Economics, London, W.C.2.
WITT-HANSEN (Hr. J.), Wesselsgade 22 III, Copenhagen N, Denmark.
WOLFENDEN (Sir John), Vice-Chancellor's Lodge, Upper Redlands Road, Reading, Berks.
WOOD (O. P.), Christ Church, Oxford.
WOODS (Rev. G. F.), Downing College, Cambridge.
WOODS (M. J.), 47 Village Way, Beckenham, Kent.

WOOLLEY (Prof. A. D.), The University, St. Andrew's, Fife, Scotland.
WORMELL (C. P.), 32 The Drive, Ricksmansworth, Herts.
WRIGHT (Prof. J. N.), The University, St. Andrew's, Fife, Scotland.
WRIGHT (P. F. J.), Temagami, 17 Ransom Road, Erdington, Birmingham.
WROE (Rev. J. P.), St. John's Seminary, Wonersh, Guildford, Surrey.

YOUNG (K.), 11 Yorkshire Road, Prestwich, Manchester.

ZEIDAN (M. F.), Trinity College, Dublin, Ireland.

(b) RESIDENT OUTSIDE EUROPE

ADAMS (Prof. E. M.), 813 Old Mill Road, Chapel Hill, N.C., U.S.A.
AD-DARZI (I. S.), P.O. Box 329, Aramco, Abquaiq, Saudi Arabia. (*Life Member.*)
ADELMAN (Miss S. L.), 1521 Sheridan Avenue, New York 57, N.Y., U.S.A.
AHLEN (Dr. A. C. M.), 3900 16th Avenue, S., Minneapolis 7, Minn., U.S.A.
AIKEN (Miss L. W.), Philosophy Department, Wellesley College, Wellesley, Mass., U.S.A.
ALDRICH (Prof. V. C.), Kenyon College, Gambier, Ohio, U.S.A.
ALLERS (Dr. R.), 1644 33rd Street N.W., Washington 7, D.C., U.S.A.
AMBROSE-LAZEROWITZ (Mrs. A.), 31 Langworthy Road, Northampton, Mass., U.S.A.
AMERMAN (J. D.), 3115 Ivy Street, Los Angeles 34, Calif., U.S.A.
AMMERMAN (R.), Philosophy Department, Bascom Hall, University of Wisconsin, Madison, Wisc., U.S.A.
ANDERSON (Prof. A. R.), 220 Canner Street, New Haven 11, Conn., U.S.A.
ANDERSON (Prof. J.), Levenshulme, 2 Nulla Nulla Street, Turramurra, N.S.W., Australia.
ANDERSON (M. G.), Philosophy Department, Trinity College, Hartford 6, Conn., U.S.A.
ANDRESEN (R. L.), 1330 Webford Avenue, Des Plaines, Ill., U.S.A.
ANGELL (Prof. R. B.), Department of Philosophy, Ohio Wesleyan University, Delaware, Ohio, U.S.A.
ANSCOMBE (Prof. F. J.), Fine Hall, Box 708, Princeton, N.J., U.S.A.
ANTON (P.), Philosophy Department, Oregon State College, Corvallis, Oregon, U.S.A.
ANTZ (Prof. Louise), 147 West 4th Street, New York 12, N.Y., U.S.A.
ARMENTI (A.), 840 East Cross Street, Ypsilanti, Mich., U.S.A.
ARMSTRONG (D. M.), Philosophy Department, Melbourne University, Carlton N.3, Victoria, Australia. (*Life Member.*)
ASCHNER (Mrs. M. J.), 1005 South 6th Street, Champaign, Ill., U.S.A.
ATKINSON (H. T.), 51 Shelbourne Street, Nelson, New Zealand.
AUNE (B.), Philosophy Department, University of Minnesota, Minneapolis 14, Minn., U.S.A.

BAIER (Prof. K.), Philosophy Department, University College, Canberra, A.C.T., Australia.
BAKER (A. J.), Department of Philosophy, The University, Sydney, Australia.
BALMUTH (J.), 52 Maple Avenue, Hamilton, N.Y., U.S.A.
BALNAVES (F. J.), State Library of W. Australia, James Street, Perth, W. Australia.
BALOWITZ (V. H.), 596 Warwick Street, Brooklyn 7, N.Y., U.S.A.
BAR-HILLEL (Y.), 30 Abarbanel Street, Jerusalem, Israel.
BARTLETT (Prof. G. R.), Box 2085 University Station, University of Florida, Gainesville, Fla., U.S.A.
BARTLEY (W. W.), 694 Green Street, Cambridge 38, Mass., U.S.A.
BAYLIS (Prof. C. A.), Duke University College Station, Durham, N.C., U.S.A.
BEALS (Prof. L. W.), Williams College, Williamstown, Mass., U.S.A.
BEARDSLEY (Prof. M. C.), 133 Rutgers Avenue, Swarthmore, Pa., U.S.A.
BECK (Dr. R. N.), 24 Mayflower Road, Leicester, Mass., U.S.A.

BECK (W. S.), M.D., Massachusetts General Hospital, Boston 14, Mass., U.S.A.
BECKNER (M. O.), Philosophy Department, Pomona College, Claremont, Calif., U.S.A.
BEDAU (H. A. Jr.), Princeton University, Princeton, N.J., U.S.A.
BEGG (J. C.), 12 Fifield Street, Roslyn, Dunedin, New Zealand.
BENEŠ (Dr. V. E.), Mendham, N.J., U.S.A.
BENJAMIN (B. S.), Philosophy Department, University College, Canberra, A.C.T., Australia.
BENSON (A. J.), 993 Santa Barbara Road, Berkeley 7, Calif., U.S.A.
BERALL (N.), Department of English, City College, Convent, Avenue Ap. 139th, New York 31, N.Y., U.S.A.
BERBERELLY (John), 159 Bagley Road, Berea, Ohio, U.S.A.
BERGMANN (Prof. G.), Philosophy Department, Iowa University, Iowa City, U.S.A.
BERMAN (Dr. Eleanor), 26 Ogden Road, Scarsdale, N.Y., U.S.A.
BERNSTEIN (Prof. R. J.), Apt. 10, 24 Sheldon Terrace, New Haven, Conn., U.S.A.
BERTOCCI (Prof. P. A.), Philosophy Department, Boston University, Boston 15, Mass., U.S.A.
BHARATI (Swami A.), Far Eastern Institute, University of Washington, Seattle 5, Wash., U.S.A.
BHARRACHARYA (Prof. S. K.), 48-B Kailas Bose Street, Calcutta 6, India.
BIERMAN (A. K.), San Francisco State College, 1600 Holloway Avenue, San Francisco 27, Calif., U.S.A.
BISHOP (Dr. P. O.), Department of Surgery, The University, Sydney, Australia.
BLACK (Prof. M.), 408 Highland Road, Ithaca, N.Y., U.S.A.
BLANSHARD (Prof. B.), Philosophy Department, Yale University, New Haven, Conn., U.S.A. (*Life Member.*)
BLANTON (R. L.), Psychology Department, College of Arts and Sciences, University of Kentucky, Lexington, Ky., U.S.A.
BLOCK (Dr. I.), 436 Eastern Parkway, Apt. 5-E, Brooklyn, N.Y., U.S.A.
BOGHOLT (Prof. C. M.), 366 Bascom Hall, University of Wisconsin, Madison, Wisc., U.S.A.
BONNEY (W. L.), Department of Philosophy, The University, Sydney, Australia.
BOUWSMA (Prof. O. K.), Burnett Hall, University of Nebraska, Lincoln, Neb., U.S.A.
BOYNTON (Prof. R. W.), 16 Irving Place, Buffalo 1, N.Y., U.S.A.
BRANDT (Prof. R. B.), Swarthmore College, Swarthmore, Pa., U.S.A.
BRAUN (E. L.), 3153 Purdue Avenue, Los Angeles 66, Calif., U.S.A.
BRETT (F. L.), 1890 East 105th Street, Cleveland 6, Ohio, U.S.A.
BRINKLEY (A. B.), 7919 Sycamore Street, New Orleans 18, La., U.S.A.
BRODBECK (Prof. M.), Philosophy Department, University of Minnesota, Minneapolis, Minn., U.S.A.
BROGAN (Dean A. P.), Graduate School, University of Texas, Austin 12, Texas, U.S.A.
BROMBERGER (S.), Philosophy Department, Princeton University, Princeton, N.J., U.S.A.
BROWN (J. H.), Philosophy Department, Pennsylvania State University, University Park, Pa., U.S.A.
BROWN (M.), Education Department, Adelphi College, Garden City, N.Y., U.S.A.
BROWN (N.), Memorial University, St. John's, Newfoundland.
BROWN (Dr. R.), Department of Social Philosophy, Australian National University, Canberra, A.C.T., Australia.
BROWNE (Prof. S. S.), University of Cincinnati, Cincinnati 21, Ohio, U.S.A.
BROYLES (J. E.), 259 Circle Drive, Moscow, Idaho, U.S.A.
BRYANT (D. W.), 164 Flora Drive, Scarborough, Ont., Canada.
BUCK (R. C.), Philosophy Department, Oberlin College, Oberlin, Ohio, U.S.A.
BURKS (Prof. A. W.), Philosophy Department, University of Michigan, Ann Arbor, Michigan, U.S.A.

BUSCHMAN (Prof. H.), University of Kansas City, Kansas City 4, Mo., U.S.A.
BUSSEY (Prof. Gertrude C.), c/o Prof. R. P. Hawes, 5412 Purlington Way, Baltimore 12, Md., U.S.A.
BUTCHVAROV (P.), Philosophy Department, University of S. Carolina, Columbia, S.C., U.S.A.

CADBURY (W.), Philosophy Department, Boston University, 236 Bay State Road, Boston 15, Mass., U.S.A.
CAIRNS (H.), National Gallery of Arts, Washington, D.C., U.S.A.
CALLAGHAN (Prof. W. J.), 835 Collingwood Drive, East Lansing, Mich., U.S.A.
CAPILDEO (S.), 25 St. Vincent Street, Port-of-Spain, Trinidad, B.W.I.
CARLSON (C. H.), 4705 West 66th Street, Minneapolis 24, Minn., U.S.A.
CARMICHAEL (Prof. P. A.), Philosophy Department, Louisiana State University, Baton Rouge 3, La., U.S.A
CARNAP (Prof. R.), Philosophy Department, University of California, Los Angeles 24, Calif., U.S.A.
CARPENTER (W.), 1455 Millet Avenue, El Monte, Calif., U.S.A.
CARRON (D. R.), 401 Larch Avenue, Bogota, N.J., U.S.A.
CARTER (W. B.), 29 Kilbarry Road, Toronto 7, Ont., Canada.
CASTAÑEDA (H. N.), Philosophy Department, Wayne State University, Detroit 2, Mich., U.S.A.
CATON (C. E.), 1103 S. Prairie Street, Champaign, Ill., U.S.A.
CELARIER (J. L.), Philosophy Department, University of Pennsylvania, Philadelphia 4, Pa., U.S.A.
CERF (Dr. W.), 444 East 17th Street, Brooklyn 26, N.Y., U.S.A.
CHAPPELL (V. C.), Philosophy Department, Swift Hall, University of Chicago, Chicago 37, Ill., U.S.A.
CHURCH (Prof. R. W.), 620 Lilac Drive, Santa Barbara, Calif., U.S.A.
CHURCHILL (J. M.), San Francisco State College, 1600 Holloway Avenue, San Francisco 27, Calif., U.S.A.
CLAPP (Prof. J. G.), Hunter College, New York, N.Y., U.S.A.
CLARK (Prof. J. W.), 35 Beverly Heights, Tuscaloosa, Alabama, U.S.A.
CLARK (R.), Philosophy Department, Duke University, Durham, N.C., U.S.A.
CLAXTON (M.), Sekondi College, P.O. Box 190, Sekondi, Ghana, W. Africa.
COATES (J.), 2700 Passyunk Avenue, Philadelphia 1, Pa, U.S.A.
COBITZ (Prof. J. L.), Philosophy Department, Brooklyn College, Brooklyn 10, N.Y., U.S.A.
COFFIN (F. H.), 67 Riverside Drive, New York 24, N.Y., U.S.A.
COHEN (S. M.), Eliot House, Cambridge, Mass., U.S.A.
COLBURN (J. B.), 1102 Tropic Lane, Santa Ana, Calif., U.S.A.
COLEMAN (Dr. W.), Smith University, Charlotte, N.C., U.S.A.
COLLINS (A. W.), 205 East 17th Street, New York 3, N.Y., U.S.A.
COLMAN (S. J.), Makerere College, Kampala, Uganda, E. Africa.
COLVER (Dr. A. W.), Division of Humanities, Fresno State College, Fresno, Calif., U.S.A.
COMEY (D. D.), 101 Orchard Place, Ithaca, N.Y., U.S.A.
CONSTABLE (Miss R.), GR 38 P.O. 10282 Time Inc., 9 Rockefeller Plaza, New York 20, N.Y., U.S.A.
COPELAND (J. W.), Philosophy Department, University of Pittsburgh, Pittsburgh, Pa., U.S.A. (*Life Member.*)
COSTELLO (Prof. H. T.), 22 Seabury Hall, Trinity College, Hartford, Conn., U.S.A.
COTTON (Prof. J. H.), Philosophy Department, Wabash College, Crawfordsville, Indiana, U.S.A.
COX (K. B.), 1519 Granger Avenue, Ann Arbor, Mich., U.S.A.
CRITTENDEN (C. C., Jr.), Philosophy Department, Cornell University, Ithaca, N.Y., U.S.A.
CROCKETT (Prof. C.), Department of Philosophy, University of Cincinnati, Cincinnati, Ohio, U.S.A.
CRUMM (D. O.), 17340 Ray, Allen Park, Mich., U.S.A.

CULBERTSON (J. T.), California State Polytechnic College, San Luis Obispo, Calif., U.S.A.
CUSHEN (Dr. W. E.), 5312 McKinley Street, Bethesda 14, Md., U.S.A.

DAAMS (Dr. G.), Philosophy Department, Kent State University, Kent, Ohio, U.S.A.
D'ARRIGO (A.), 706 So. Illinois Avenue, Carbondale, Ill., U.S.A.
DALEY (J. J.), 29 Lorraine Street, Glen Ridge, N.J., U.S.A.
DANTO (A. C.), Philosophy Department, Columbia University, New York 27, N.Y., U.S.A.
DAVIDSON (D.), Philosophy Department, Stanford University, Stanford, Calif., U.S.A.
DAVIS (Dr. J. W.), Philosophy Department, University of W. Ontario, London, Ont., Canada.
DAVIS (R. R.), 9 Edgehill Drive, Alexandria, Virginia, U.S.A.
DEAN (Prof. L. F.), 6 Bellevue Avenue, Winchester, Mass., U.S.A.
DE BOER (Prof. J.), Philosophy Department, University of Kentucky, Lexington, Ky., U.S.A.
DE GRAAFF (G. E.), Queen's College, Melbourne University, Carlton N.3, Victoria, Australia.
DE MARNEFFE (Rev. J.), De Nobili College, Poona 6, India.
DEMOS (Prof. R.), Emerson Hall, Harvard University, Cambridge, Mass., U.S.A.
DENISE (T. C.), Department of Philosophy, Syracuse University, Syracuse, N.Y., U.S.A.
DENNES (Prof. W. R.), University of California, Berkeley, Calif., U.S.A.
DESHPANDE (D. Y.), Vidarbha Mahavidyalaya, Amravati, Bombay State, India.
DIAMADOPOULOS (P.), Philosophy Department, University of Maryland, College Park, Md., U.S.A.
DIGIOVANNI (C.), 720 Newton Road, Ithan, Villanova, Pa., U.S.A.
DOESCHER (W. O.), 2406 Havenwood Drive N., Columbus 9, Ohio, U.S.A.
DOLPHIN (V. M.), Arizona State College, Tempe, Arizona, U.S.A.
DONAGAN (Prof. A. H.), Philosophy Department, University of Minnesota, Minneapolis 14, Minn., U.S.A.
DONEY (Prof. W.), Philosophy Department, Dartmouth College, Hanover, New Hampshire, U.S.A.
DOYLE (Ensign L.), BIG-3s BOQ, NAAS Whitney Field, Milton, Florida, U.S.A.
DRAY (W.), Philosophy Department, University of Toronto, Toronto, Ont., Canada.
DREBEN (B. S.), 30 Hillside Terrace, Belmont, Mass., U.S.A.
DRYER (Prof. D. P.), University College 48-W, The University, Toronto, Ont., Canada.
DUCASSE (Prof. C. J.), Brown University, Providence, R.I., U.S.A.
DUNCAN (Prof. A. R. C.), Queen's University, Kingston, Ont., Canada.
DUNCAN (M.), 2213 Bandywood Drive, Nashville 12, Tenn., U.S.A.
DUNHAM (Prof. B.), 127 Bentley Road, Cynwyd, Pa., U.S.A.
DURRANT (R. G.), Philosophy Department, University of Otago, P.O. Box 56, Dunedin, New Zealand.

EAGLESON (C.), 119 E. Durham Street, Philadelphia 19, Pa., U.S.A.
EBERSOLE (Prof. F. B.), San Jose State College, San Jose 14, Calif., U.S.A.
ECKSTEIN (W.), 3900 Greystone Avenue, New York 63, N.Y., U.S.A.
EDEL (Prof. A.), Department of Philosophy, City College of New York, New York 31, N.Y., U.S.A.
EDRINGTON (D. and K.), Route No. 1, Box 36A, Gig Harbor, Washington, U.S.A.
EDWARDS (Prof. P.), Philosophy Department, Washington Square College, Washington Square, New York 3, N.Y., U.S.A.
EDWARDS (R. B.), 409 Prospect Street, New Haven, Conn., U.S.A.

EKMAN (Miss R.), 61 Paradise Road, Northampton, Mass, U.S.A.
ELTON (Prof. W.), Division of Humanities, University of California, Riverside, Calif., U.S.A.
EMERY (Prof. S. A.), Department of Philosophy, University of North Carolina, Chapel Hill, N.C., U.S.A.
EPSTEIN (Prof. J.), 17 Walnut Street, Amherst, Mass., U.S.A.
ERICKSON (R. W.), College Station, Columbus, Miss., U.S.A.
ESTALL (Prof. H. M.), Queen's University, Kingston, Ont., Canada.
EVERALL (Dr. D. G.), City College of San Francisco, Balboa Park, San Francisco 12, Calif., U.S.A.

FAIN (H.), Philosophy Department, 372 Bascom Hall, University of Wisconsin, Madison 6, Wisc., U.S.A.
FEE (J. H.), 1072 Fiesta Drive, San Mateo, Calif., U.S.A.
FEIBLEMAN (Prof. J. K.), 1424 Canal Building, New Orleans 12, La., U.S.A.
FEIGL (Prof. H.), Philosophy Department, University of Minnesota, Minneapolis, Minn., U.S.A.
FELCH (W. E.), Findlay College, Findlay, Ohio, U.S.A.
FERGUSON (J. P.), Fourah Bay College, Freetown, Sierra Leone, W. Africa.
FERRE (Prof. F. P.), Philosophy Department, Vanderbilt University, Nashville 5, Tennessee, U.S.A.
FICARRA (F.), Chico State College, Chico, Calif., U.S.A.
FINCH (R.), 106 Liberty Avenue, New Rochelle, N.Y., U.S.A.
FINNIS (M. M. S.), The University, Adelaide, S. Australia.
FIRTH (Prof. R.), Emerson Hall, Harvard University, Cambridge, Mass., U.S.A.
FISCH (Prof. M. H.), 322 Gregory Hall, University of Illinois, Urbana, Ill., U.S.A.
FISCHER (Prof. K. R.), 2451 Webster Street, Berkeley 5, Calif., U.S.A.
FITCH (Prof. F. B.), 1834 Yale Station, New Haven, Conn., U.S.A.
FLOWER (Prof. Elizabeth), 218 South De Kalb Street, Philadelphia 4, Pa., U.S.A.
FOX (Dr. M.), Philosophy Department, Ohio State University, Columbus 10, Ohio, U.S.A.
FRANCK (I.), 2605 Ross Road, Chevy Chase 15, Md., U.S.A.
FRANKEL (Prof. C.), Phillips Hill Road, New City, N.Y., U.S.A.
FRASER (Lt. Cdr. J. L.), 150 Driveway, Ottawa, Canada.
FREED (B.), c/o Mrs. Lola White, Gambier, Ohio, U.S.A.
FRENCH (Prof. S. G.), Philosophy Department, University of Western Ontario, London, Ont., Canada.
FRY (E. M.), c/o North Broken Hill Ltd., Broken Hill, N.S.W., Australia.

GABRIEL (Mrs. A. M.), 2 Balfour Street, Dunedin N.E.1, New Zealand.
GAINES (R. J.), Apt. 7A, 233 Prospect Street, East Orange, New Jersey, U.S.A.
GALLIGAN (E. M.), Philosophy Department, Indiana University, Bloomington, Ind., U.S.A.
GANGULI (Prof. S.), 13/A Raja Dinendra Street, Calcutta 9, India.
GARCIA (Sr R. V.), Acevedo 2231, DTO 7, Buenos Aires, Argentina.
GARNETT (Prof. A. C.), 191 Bascom Hall, University of Wisconsin, Madison, Wisc., U.S.A.
GARVER (J. N.), Philosophy Department, University of Minnesota, Minneapolis 14, Minn., U.S.A.
GERLACH (Mrs. M. W.), 527 Las Alturas Road, Santa Barbara, Calif., U.S.A.
GERRISH (Dr. B. A.), 2330 North Halsted Street, Chicago 14, Ill., U.S.A.
GERT (B.), 862 East Mitchell Avenue, Cincinnati 29, Ohio, U.S.A. (*Life Member.*)
GETTIER (E. L.) III, 1661 Burlingame Avenue, Detroit 6, Mich., U.S.A.
GEWIRTH (Prof. A.), Department of Philosophy, University of Chicago, Chicago 37, Ill., U.S.A.
GEYER (Prof. D. L.), University Club, 2304 San Antonio Street, Austin 21, Tex., U.S.A.

GIESCHEN (D.), 5722 N. Kenmore Avenue, Chicago 40, Ill., U.S.A.
GILPIN (Prof. R. C.), Philosophy Department, Butler University, Indianapolis 7, Indiana, U.S.A.
GINET (C.), Philosophy Department, Ohio State University, Columbus 10, Ohio, U.S.A.
GINZBERG (M. L.), 36 Gramercy Park East, New York 3, N.Y., U.S.A.
GLASSEN (P.), Philosophy Department, University of Manitoba, Winnipeg, Canada.
GODDARD (L.), Philosophy Department, University of New England, Armidale, N.S.W., Australia.
GOHEEN (Prof. J. D.), Philosophy Department, Stanford University, Stanford, Calif., U.S.A.
GOLDING (M. P.), 260 Riverside Drive, New York 25, N.Y., U.S.A.
GOODMAN (Prof. N.), Schwenksville, Pa. (R. D.), U.S.A.
GORDON (M. B.), 63-61 Yellowstone Blvd., Forest Hills, N.Y., U.S.A.
GOTESKY (Prof. R.), University of Georgia, Athens, Ga., U.S.A.
GOTSHALK (D. W.), 402 W. Iowa Street, Urbana, Ill., U.S.A.
GOUDGE (Prof. T. A.), University of Toronto, Toronto 5, Ont., Canada.
GRANT-SUTTIE (R. J.), c/o Messrs C. Hoare & Co., 37 Fleet Street, London, E.C.4.
GRAVE (S. A.), Philosophy Department, University of W. Australia, Nedlands, W. Australia.
GROSS (M. W.), Rutgers University, New Brunswick, N.J., U.S.A.
GRUEN (Prof. W.), New York University, Washington Square, New York 3, N.Y., U.S.A.
GUNDERSON (K. R.), c/o The Graduate College, Princeton University, Princeton, N.J., U.S.A.
GUTMANN (Prof. J.), Columbia University, New York 27, N.Y., U.S.A.

HALBERSTADT (W. H.), Philosophy Department, University of Nevada, Reno, Nevada, U.S.A.
HALL (Prof. E. W.), University of North Carolina, Chapel Hill, N.C., U.S.A.
HALLIE (Prof. P. P.), Philosophy Department, Vanderbilt University, Nashville, Tenn., U.S.A.
HAMBLIN (Mrs. F. D.), 95 Canterbury Road, Rochester 7, N.Y., U.S.A.
HANRAHAN (J. F.), 26 Pemberton Square, Boston, Mass., U.S.A.
HANSON (Prof. N. R.), Philosophy Department, Indiana University, Bloomington, Indiana, U.S.A.
HARDIE (Prof. C. D.), The University, Hobart, Tasmania.
HARDY (Dr. Helen), Philosophy Department, Trinity College, Toronto 5, Ont., Canada.
HARGROVE (T.), 20 Romolo Place, San Francisco 11, Calif., U.S.A.
HARMAN (G. H.), 224 Indian Creek Road, Philadelphia 31, Pa., U.S.A.
HARRIS (Prof. Marjorie S.), Randolph-Macon Woman's College, Lynchburg, Va., U.S.A.
HART (Prof. S. L.), 573 Grenville Avenue, Teaneck, N.J., U.S.A.
HARTLAND-SWANN (Dr. J. J.), University of Malaya, Singapore, Malaya.
HARTMAN (Prof. R. S.), Copilco 195, Mexico 20, D.F., Mexico.
HARTT (Prof. J.), 409 Prospect Street, New Haven, Conn., U.S.A.
HARVEY (H. H. III), 936 St. Charles Street, New Orleans 12, La., U.S.A.
HASEROT (Prof. F. S.), 145 Marlborough Road, Asheville, N.C., U.S.A.
HATCH (L. S., Jr.), 234 East Street, North Attleboro, Mass., U.S.A.
HAY (Prof. W. H.), 39 Bagley Court, Madison 5, Wisc., U.S.A.
HEINTZ (J. W.), 922½ Lancaster Street, Durham, N.C., U.S.A.
HELMERS (F. J.), Briarcliff College, Briarcliff Manor, N.Y., U.S.A.
HENDERSON (Prof. T. G.), 1556 Summerhill Avenue, Montreal, Canada.
HENRY (Prof. C. F. H.), 1014 Washington Building, Washington 5, D.C., U.S.A.
HENZE (D. F.), Philosophy Department, North Illinois University, De Kalb, Ill., U.S.A.
HERON (D. B.), Department of Social Philosophy, Australian National University, Canberra, A.C.T., Australia.

HICK (Rev. J. H.), Sage School of Philosophy, Cornell University, Ithaca, N.Y., U.S.A.
HILL (Prof. T. E.), Macalester College, St. Paul 5, Minn., U.S.A.
HINSHAW (V., Jr.), Dept. of Philosophy, Ohio State University, Columbus 10, Ohio, U.S.A.
HINTZ (Prof. H.), Philosophy Department, Brooklyn College, Brooklyn 10, N.Y., U.S.A.
HOAG (R. G.), 145 Pinckney Street, Apt. 704, Boston, Mass., U.S.A.
HOCKING (Prof. W. E.), Madison, N.H., U.S.A.
HOEKSTRA (Prof. R.), Philosophy Department, Wayne University, Detroit 1, Mich, U.S.A.
HOFFMAN (R.), 2273 Parkhurst Road, Elmont, L.I., N.Y., U.S.A.
HOLMER (Prof. P. L.), 100 Wesbrook Hall, University of Minnesota, Minneapolis 14, Minn., U.S.A.
HOOK (Prof. S.), Dept. of Philosophy, New York University, Washington Square College, Washington Square, New York, N.Y., U.S.A.
HOSPERS (Prof. J. J.), Philosophy Department, Brooklyn College, Brooklyn 10, N.Y., U.S.A.
HUGGETT (W. J.), Philosophy Department, University of Manitoba, Winnipeg, Manitoba, Canada.
HUGHES (Prof. G. E.), Victoria University College, Wellington, New Zealand.
HUNGERLAND (Prof. H.), 71 Dudley Avenue, Piedmont 11, Calif., U.S.A.
HUTCHESON (R.), 457 Park Avenue, Meadville, Pa., U.S.A.

ISENBERG (A.), Philosophy Department, Stanford University, Stanford, Calif., U.S.A.
IVINS (W. M. Jr.), Woodbury, Conn., U.S.A.

JACK (Dr. H. H.), Museum 106, University of Kentucky, Lexington, Ky., U.S.A.
JOHNSON (A. H.), Philosophy Department, University of Western Ontario, London, Ont., Canada.
JOHNSON (Rev. G.), 706 Nottingham Road, Wilmington 5, Del., U.S.A.
JOHNSTONE (H. W. Jr.), Philosophy Department, Pennsylvania State College, State College, Pa., U.S.A.
JOSSELYN (J. R.), 2A Rue Roy, Lauzon, Lévis, Quebec, Canada.
JUAREZ-PAZ (R.), Philosophy Department, Sparks Bdg., Pennsylvania State University, University Park, Pa., U.S.A.

KALISH (D.), Philosophy Department, University of California, Los Angeles 24, Calif., U.S.A.
KAMINS (H.), Philosophy Department, Reed College, Portland, Oregon, U.S.A.
KAMINSKY (Dr. J.), 103 Chestnut Street, Binghamton, N.Y., U.S.A.
KEGLEY (Prof. C. W.), Wagner College, Staten Island 1, New York, N.Y., U.S.A.
KERBY-MILLER (S.), 122 Girjalva Drive, San Francisco 27, Calif., U.S.A.
KERNER (G.), Philosophy Department, University of Nevada, Reno, Nevada, U.S.A.
KESSLER (Prof. H.), 603 W. Indiana Street, Urbana, Ill., U.S.A.
KEYT (D. A.), Philosophy Department, University of Washington, Seattle, Wash., U.S.A.
KEYWORTH (D. H.), Drake University, Des Moines, Iowa, U.S.A.
KHATCHADOURIAN (H.), Philosophy Department, American University, Beirut, Lebanon.
KIEFFER (J. S.), 2 Cumberland Court, Annapolis, Md., U.S.A.
KITELEY (M. J.), Philosophy Department, San Jose College, San Jose, Cal., U.S.A.
KLAUSSNER (N. W.), Grinnell College, Grinnell, Iowa, U.S.A.
KOHL (M.), 1972 W. Hon. Avenue, Bronx 53, N.Y., U.S.A.
KOLENDA (Prof. K.), Philosophy Department, Rice Institute, Houston, Texas, U.S.A.

KONVITZ (Prof. M. R.), 53 Einstein Drive, Princeton, N.J., U.S.A.
KOWAL (H.), National Labour Relations Board, 24 School Street, Boston 8, Mass., U.S.A.
KOWIT (S.), 1761 E. 14th Street, Brooklyn 29, N.Y., U.S.A.
KRAMER (A.), Rosedale, 30 Walmer Road, Woodstock, Cape Town, S. Africa.
KRETZMANN (N.), Philosophy Department, Ohio State University, Columbus 10, Ohio, U.S.A.
KRUSÉ (Prof. C.), Philosophy Department, Wesleyan University, Middletown, Conn., U.S.A.
KRZYWICKI (Dr. G.), 18 E. 64th Street, New York 21, N.Y., U.S.A.
KUHNS (R. F., Jr.), 420 Riverside Drive, New York 25, N.Y., U.S.A.
KUIPER (Prof. J.), 107 Carnegie Bldg., University of Kentucky, Lexington 29, Ky., U.S.A.
KUNTZ (Dr. P. G.), 1321 Main Street, Grinnell, Iowa, U.S.A.
KURTZ (Prof. P. W.), Trinity College, Hartford 6, Conn., U.S.A.

LADD (Prof. J.), 153 Bowen Street, Providence 6, R.I., U.S.A.
LAMPEL (Miss C.), 458 Oriole Parkway, Toronto, Ont., Canada.
LARSEN (Prof. R. E.), Philosophy Department, San Jose State College, San Jose, Calif., U.S.A.
LARSON (Miss S.), 159 Clifford Terrace, San Francisco 17, Calif., U.S.A.
LAWRENCE (Prof. N.), Little Meadow Road, Guildford, Conn., U.S.A.
LAWRENCE (R. F.), 308 Parnassus Avenue, San Francisco 22, Calif., U.S.A.
LAWSON (J. B.), 141 Dogwood, Park Forest, Ill., U.S.A.
LAWTON (C. D.), 11 Buswell Street, Boston 15, Mass., U.S.A.
LEAN (Prof. M. E.), Philosophy Department, Brooklyn College, Brooklyn 10, N.Y., U.S.A.
LEAVENWORTH (Mrs. I.), 277 Park Avenue, New York 17, N.Y., U.S.A.
LEE (Prof. H. N.), Philosophy Department, Newcomb College, New Orleans 18, La., U.S.A.
LEISER (B. M.), 30 Harrison Street, Taunton, Mass., U.S.A.
LEMOS (R. M.), Philosophy Department, University of Miami, Coral Gables, Florida, U.S.A.
LENSING (W. E.), Bard College, Annandale-on-Hudson, N.Y., U.S.A.
LEONARD (Prof. H. S.), Philosophy Department, Michigan State University, East Lansing, Mich., U.S.A.
LEVING (L.), Box 505, Stanford, Calif., U.S.A.
LEVINSON (Mr. H.), 1732 West Third Street, Brooklyn 23, N.Y., U.S.A.
LEWIS (Prof. C. I.), 68 Yale Road, Menlo Park, Calif., U.S.A.
LEYS (Prof. W. A. R.), 715 Washington Avenue, Wilmette, Ill., U.S.A.
LICHTIGFELD (Dr. A.), Arcadia, 22 Oxford Road, Parktown, Johannesburg, South Africa.
LIDDELL (Miss A. F.), 647 West Pensacola Street, Tallahassee, Fla., U.S.A.
LIDDELL (B. E. A.), 208 No. Division, Apt. 4, Ann Arbor, Mich., U.S.A.
LIEB (I.), Philosophy Department, Connecticut College, New London, Conn., U.S.A.
LINDLEY (Dr. T. F.), U-54, University of Connecticut, Storrs, Conn., U.S.A.
LINNELL (Dr. J.), Philosophy Dept., Sacramento State College, Sacramento, Calif., U.S.A.
LLEWELYN (J. E.), Philosophy Department, University of New England, Armidale, N.S.W., Australia.
LOGAN (Prof. R. L.), Philosophy Department, Texas Christian University, Fort Worth, Texas, U.S.A.
LOW-BEER (F.), 4799 Drummond Drive, Vancouver, British Columbia, Canada. (*Life Member.*)
LUCE (D. R.), Philosophy Department, University of Arkansas, Fayetteville, Ark., U.S.A.
LUNSFORD (C. W.), 21903 Duncan Avenue, Riverside, Calif., U.S.A.
LUSCHEI (E. C.), P.O. Box 53, A.P.O. 742, New York, N.Y., U.S.A.

McAllister (W. K.), Howard University, Washington 1, D.C., U.S.A.
McCall (Dr. G. R.), 2777 Hill Park Circle, Montreal, Canada.
McCallum (D.), Department of Moral and Political Philosophy, The University, Sydney, Australia.
McCarthy (T. W.), 421 Kedzie Street, Evanston, Ill., U.S.A.
McCloskey (Dr. H. J.), Philosophy Department, Melbourne University, Carlton N.3, Victoria, Australia.
McClung (M.), 460 Golden Avenue, Ottawa, Canada.
McDermut (W.), 715 Anderson Avenue, Cliffside, N.J., U.S.A.
Macdonald (Prof. L. D.), Philosophy Department, University of Wichita, Wichita 14, Kansas, U.S.A. (*Life Member.*)
McGechie (J. E.), Philosophy Department, University of Melbourne, Carlton N.3, Victoria, Australia.
McGee (Prof. C. D.), Philosophy Department, Vassar College, Poughkeepsie, N.Y., U.S.A.
McGuire (J. E.), 1276 Langmuir Avenue, London, Ont., Canada.
McIntosh (G. F.), Philosophy Department, New England University College, Armidale, N.S.W., Australia. (*Life Member.*)
McKenzie (A. G.), 6 Royal Crescent, Woonona 5c, N.S.W., Australia.
McKenzie (Prof. L. W., Jr.), Department of Economics, University of Rochester, Rochester, N.Y., U.S.A.
McKeon (Prof. R. P.), University of Chicago, Chicago 37, Ill., U.S.A.
McLellan (J. M.), Philosophy Department, Western Reserve University, Cleveland 6, Ohio, U.S.A.
McMurrin (Prof. S. M.), University of Utah, Salt Lake City 1, Utah, U.S.A.
Magel (Mr. C. R.), 509 7th Avenue, Minneapolis 14, Minn., U.S.A.
Magrish (J. L.), 921 Dixie Terminal Building, Cincinnati 2, Ohio, U.S.A.
Main (A. N.), 5668 Gato Avenue, Goleta, Calif., U.S.A.
Malcolm (Prof. N.), Philosophy Department, Cornell University, Ithaca, N.Y., U.S.A.
Maltzman (I.), Psychology Department, University of California, Los Angeles 24, Calif., U.S.A.
Mandelbaum (Prof. M. H.), 3700 North Charles Street, Baltimore, Md., U.S.A.
Marchment (A. R.), 202 Van Dusen Blvd., Toronto 18, Ont., Canada. (*Life Member.*)
Marcus (Dr. Ruth B.), Roosevelt University, 430 S. Michigan Avenue, Chicago, Ill., U.S.A.
Mardiros (Prof. A. M.), University of Alberta, Edmonton, Alberta, Canada.
Margold (L. A.), 11 Seacord Road, New Rochelle, N.Y., U.S.A.
Marks (R. L.), Kingstown, St. Vincent, British West Indies.
Markwell (D. S.), P.O. Box 274, Timaru, New Zealand.
Maron (Dr. M. E.), 2210 Roscomare Road, West Los Angeles 24, Calif., U.S.A.
Marsh (R. C.), Apt. 206, 7020 Jeffery Blvd., Chicago 49, Ill., U.S.A.
Marshall (Prof. J. S.), University of the South, Sewanee, Tenn., U.S.A.
Martin (Prof. R. M.), Philosophy Department, University of Pennsylvania, Philadelphia 4, Pa., U.S.A.
Marutollo (F.), 4248 Boyd Avenue, New York 66, N.Y., U.S.A.
Marvin (Prof. E. L.), Montana State University, Missoula, Mon., U.S.A.
Masani (B. M.), Evergreen, Gholvad, Thana District, Bombay Province, India.
Maslow (Prof. A.), 1684 Acadia Road, Vancouver 8, British Columbia, Canada. (*Life Member.*)
Mason (H. E.), Philosophy Department, University of Minnesota, Minneapolis 14, Minn., U.S.A.
Matheson (G. F.), School of Philosophy, University of S. California, Los Angeles 7, Calif., U.S.A.
Matson (Prof. W. I.), Philosophy Department, University of California, Berkeley 4, Calif., U.S.A.
Matthews (G.), 947 So. Fedora, Los Angeles 6, Cal., U.S.A.
Matthews (G. B.), 28 Irving Street, Apt. 2, Cambridge 38, Mass., U.S.A.

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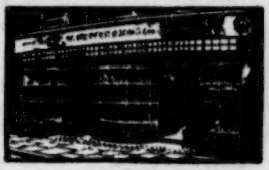

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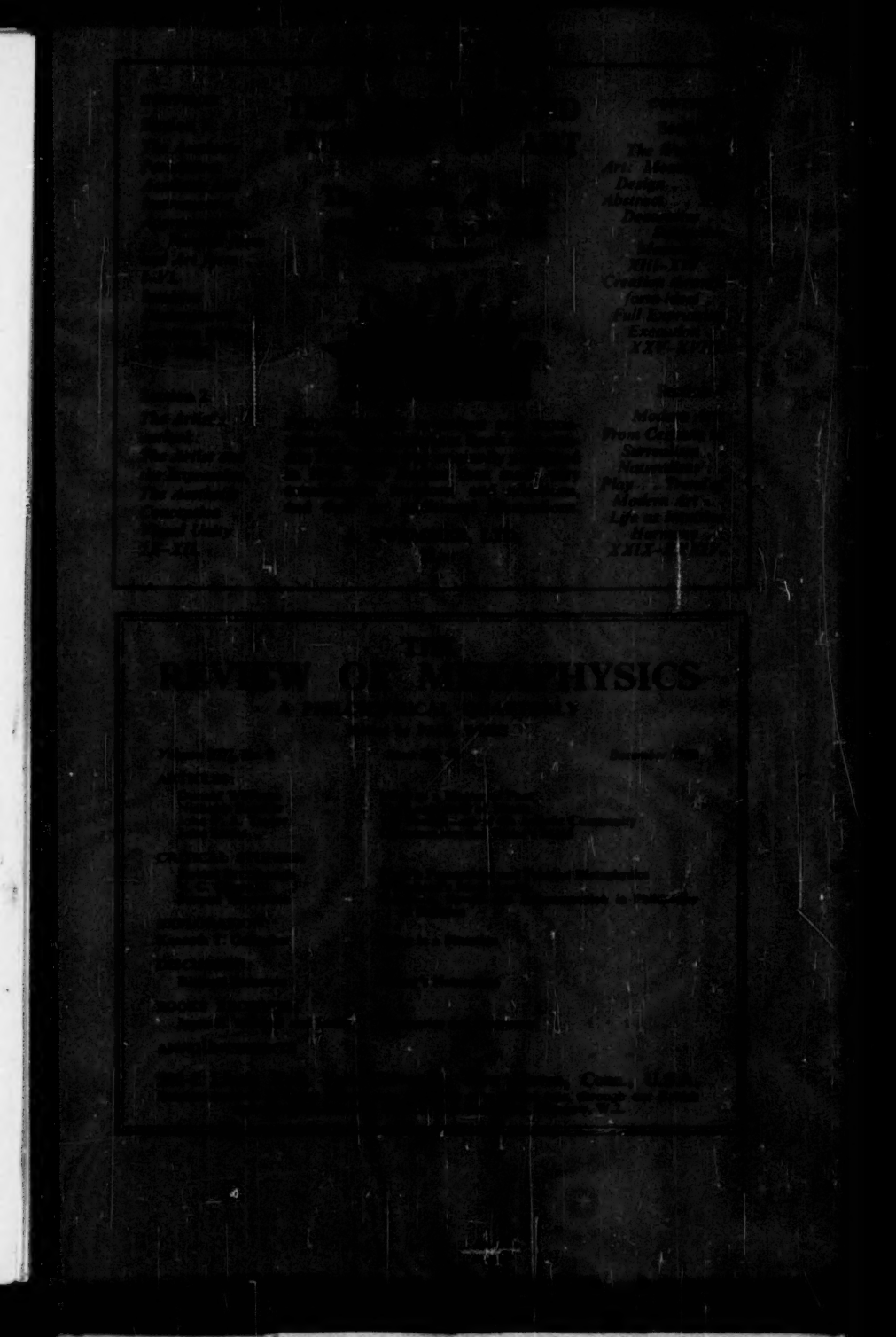

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